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627

"STROBOSCOPY."

By Dr. C. V. Drysdale.

Read before the Optical Society on November 16th, 1905.

When considering the most suitable subject to bring before the Society this evening, it struck me that some work which I have recently been engaged on, though not undertaken with an optical object, might interest you. In the electrical engineering laboratories of this Institute we have experienced great trouble from time to time in the accurate measurement of the speeds of our dynamos and motors, and this difficulty, coupled with others, has led me to take up the matter from the optical side with satisfactory results. It is my purpose this evening to say something about the stroboscopic measurements which I have been working on, and at the same time to show of what varied and valuable uses the stroboscope is capable.

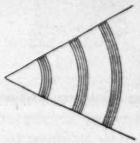

FIG. 1.

FIG. 5.

FIG. 6.

In the first place, what is a stroboscope? The Encyclopædia Britannica, Chamber's Encyclopædia, and Lloyd's Dictionaries are silent on the subject. In the Century Dictionary I find that the word stroboscope is derived from the Greek στροβος twist, and σκοπειν view and it is defined as "an instrument used in the study of the periodic motion of a body, as one in rapid revolution or vibration by illuminating it at frequent intervals . . . or, again, by viewing it through the openings of a revolving disc."

The simplest form of stroboscope is, in fact, a whirling disc perforated with a number of holes or slits, through which a moving body is either viewed directly or by illumination through these apertures. The ordinary Zoëtrope, or wheel of life, is an example of this.

In its more extended meaning, however, a stroboscope is any device whereby an object is rendered visible by brief flashes at regular intervals, these flashes being produced either by a rotating slotted disc, or electric contact, a tuning fork or vibrating body, or by an arc lamp. In fact, a stroboscopic beam of light is any beam which is cut up into a series of regular flashes, as in fig. 1, depicting sets of waves moving outwards from a source with dark spaces between.

The properties of such a beam are most interesting and valuable, as it renders it possible for various

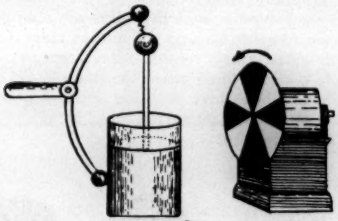

FIG. 2.

phenomena to be observed, of which we should otherwise have no evidence, and to make several measurements with accuracy and convenience. I now propose to show you experimentally some of the effects of such a stroboscopic beam, and the observations which can be made with its aid, and afterwards the measurements which can be undertaken with it.

In the first place I have a beam of ordinary light projected upon the screen, and whenever a rapidly moving object is viewed by this light it appears blurred and unrecognisable. But if I cause this beam of light to be shown as a flash the moving objects appear stationary, as in instantaneous photography. The most effective way of doing this is by obtaining a bright spark from a Leyden jar, which gives such an extremely

short and brilliant flash that Professor Boys was able to photograph bullets in their flight. I have here a disc with black and white sectors in rapid rotation, thus appearing a uniform grey tint by ordinary light, but on illuminating it with the spark from a Leyden jar it appears absolutely stationary, with the edges of the sectors apparently as sharp as if there were no motion.

Now, if instead of a single flash, we have a series of such flashes at regular intervals, and view by means of them a moving body, at each of these flashes the body must appear fixed in position, and if the flashes are very frequent, the persistence of vision will cause us to see several of these positions together. For example, if I move this pointer backwards and forwards in front of the screen it appears blurred (fig. 3), but on doing the same thing with a beam made up of rapid flashes (fig. 4) I appear to be holding a sheaf of pointers.

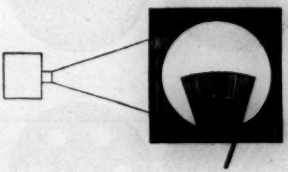

FIG. 3.

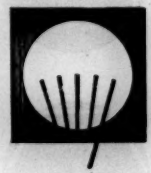

FIG. 4.

The same thing is better shown by this rotating disc having a white line on it (fig. 5), or by these other ones with a dot or tangential line. On viewing these discs when slowly rotating with the stroboscope beam we observe that instead of one dot or line we see several quite clearly (fig. 6).

But now let us increase the speed of the disc further and we shall see that the dot becomes a regular series, the radial line appears like the spokes of a wheel, or the tangential line becomes a polygonal figure. If, for example, in the time between one flash and the next the disc turns exactly one sixth of a revolution, we shall apparently have a perfectly stationary ring of six dots, or a wheel with six arms, or a hexagon, as in fig. 7. If the disc turns at a slightly lower rate the dots or lines will not have advanced quite one-sixth of a revolution

between the flashes, and the result will be to see the same pattern as before, but it will appear to be rotating slowly backwards, as compared with the real rotation of the disc.

On the other hand, if the speed of the disc is slightly greater than that first mentioned the rotation between the flashes will be a little more than one-sixth, and the pattern will appear to move forwards.

When the speed of rotation is, therefore, any definite fraction of the rapidity of the flashes, a stationary pattern is produced, the number of dots, spokes, or sides, of which is equal to the number obtained by dividing the

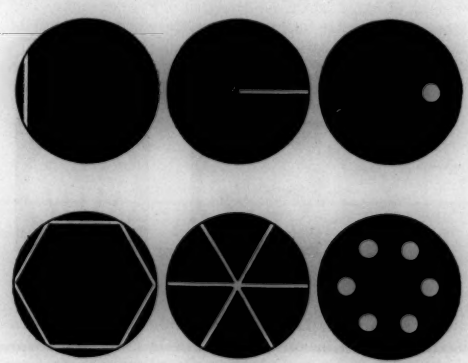

FIG. 7.

number of flashes by the number of revolutions per minute. In each case a small increase or decrease of the speed of the disc causes the pattern to rotate backwards or forwards.

The pattern produced in this way is not very bright, and for a simple reason. Not only is the illumination small owing to the illumination being only in flashes instead of continuous, but as the same dot or line is made to appear in several different places the illumination of each of the images is only a fraction of that which it would have were it stationary, the fraction being the same as the number of dots, spokes, or sides

seen. But we may get over this by making use of a pattern having any definite number of dots, spokes, or sides, so long as they are all similar.

If, for example, we have a ring of six dots or a wheel with six arms, or a figure with six sides (a hexagon), and rotate it at the same speed as before, the disc appears as if stationary, and the image is as bright as when this was the case. Again, as before, if the rotation is slower or quicker than the exact fraction the pattern appears to move more or less slowly backwards or forwards.

An amusing instance of this is not infrequently to be seen in the now familiar living pictures. As we all know these are produced by taking a number of instantaneous photographs at regular intervals, just as with our stroboscopic beam. Consequently, if a photograph is taken of a moving cart or wheel of any kind in which the spokes are all alike, and this wheel is moving at such a speed that one spoke exactly gets into the position of some other in the time between the photographs, the wheel will not appear to have turned, and the cart will move across the screen with apparently stationary wheels.

But, further, if the speed is less than this particular value we must have the ridiculous spectacle of a cart moving forwards with its wheels actually turning in the wrong direction. This must have been witnessed by most of you several times, but in order that you shall see it in the best possible manner, Mr. Paul has very kindly consented to show you four films in his animatograph, which films he has specially selected as showing these effects most clearly. You will see that as the various vehicles pass across the screen the wheels appear in many cases to be sliding instead of rolling, and in some instances they actually appear to move backwards. I hope you will join with me in thanking Mr. Paul and his manager, Mr. Smith, for arranging this demonstration.

We have, therefore, seen that one of the first effects of a stroboscopic beam is that it may apparently bring a rotating body to react or reduce its apparent speed so that its rotation may be conveniently observed. The same thing must evidently apply to anything which moves rapidly in such a way as to continually repeat the same movements. Such a motion is termed a periodic motion, and a good example of it is the motion of the

piston of an engine, or the vibration of a spring or stretched string.

By viewing such a vibrating body through the slits of a disc revolving on a motor shaft, or projecting light through these slits upon it, the vibrations may be apparently brought to rest at any part of the period or made to go through their changes as slowly as we please, so that we can examine in detail each stage of the vibration and the forms which the body successively assumes. For example, I throw the beam of light upon this

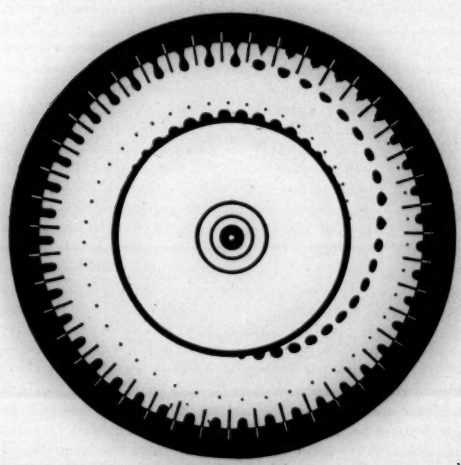

FIG. 8.
TAKEN FROM PROFESSOR BOYS' "SOAP BUBBLES."

vibrating string, and as the speed of the flashes gets greater and greater the vibrations of the cord seem to get slower, until it seems to wag backwards and forwards like a strip bent by hand.

Again, on throwing the beam on this instrument known as a frequency meter consisting of a number of reeds, some of which are in vibration with an alternating current, and, consequently, seem blurred, the reeds

appear to move backwards and forwards quite slowly, and it is very interesting to notice that although the various reeds move at the same rate, they do not move together; in fact, some lag behind and others lead in front of the force, as we know should be the case from mathematical considerations.

The next great sphere for stroboscopy is when a series of bodies follow one another through regular changes of position and conformation. In this case, we may, by means of a stroboscope, apparently destroy or reduce considerably the changes of position, leaving only the changes of shape to be observed. This is well seen in the zoëtrope, or wheel of life, or the praxinoscope, or phenakistoscope, which were the forerunners of our modern moving pictures, and were popular as toys before the advent of the latter.

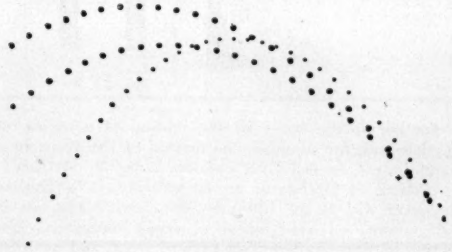

FIG. 9.

By drawing a figure at regular intervals round a disc or cylinder, and making that figure assume various successive changes of shape, we were able to see people dancing, jugglers throwing balls, etc., by viewing these figures through slits in the revolving disc or cylinder. I have an interesting scientific example of this in the zoëtrope, or thaumathrope, included with Professor Boys' charming book on "Soap Bubbles," and when this is revolved we see apparently the formation of a drop of water falling and splashing in the water below (fig. 8).

I have now said as much as is desirable about the general effects of stroboscopy, and I now come to the

more important question. What are the practical uses of the stroboscope? These may be divided into two classes—observation and measurement.

Whenever we have a periodic phenomenon, that is, one which continually repeats the same changes at regular intervals, however rapid, observation by the stroboscope enables us to see these changes going on as slowly as we please, or to apparently arrest the change at any desired epoch. The scientific uses of this are very numerous, and two or three illustrations will here be given. One of these is the investigation of the motion of string or other vibrating bodies, as we have already seen.

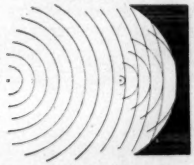

FIG. 10.

FIG. 11.

Another is the study of the motion of a series of particles, as, for example, the motion of the drops in a fountain, etc., so beautifully shown in fig. 9. As this is a meeting of Opticians, an illustration of a similar character will be of interest. In recent times, as is well known, we have learnt to study optics as the motion of waves rather than rays of light, and we have frequently illustrated the behaviour of these waves by means of ripples set up in water or other liquids.

One of the difficulties of this illustration is, that these ripples move so quickly that it is inconvenient for us to follow them and explain their motion. If, however, we have the ripples excited by a small vibrating point and illuminate them by a stroboscopic beam, we can make these ripples apparently move quite slowly, or even keep them stationary at a reflecting or refracting surface, so as to see exactly what occurs (fig. 10).

As a last example of scientific stroboscopic observation, I would refer to the stroboscopic examination of the electric arc carried out by Trotter, Blondel, Hospitalier, and others. If we take an ordinary electric

arc between two carbons produced by an alternating current, and project its image on the screen, we see the two carbon points white hot with a purple flame, or arc, between them.

On interposing a stroboscopic disc in the beam, however, and running it up to a suitable speed, we observe fluctuations in the brightness, and presently it is clearly apparent that the white patch shifts backwards and forwards from one tip to the other, and that the flame is formed and extinguished periodically (fig. 11). Rapidly, therefore, though the current changes (100 reversals per second) it is evident that there is time for the tip of the carbon to get white hot and to cool appreciably, and the bright patch changes from one carbon to the other as the current reverses. By putting a magnet near the arc we see the flame shifting from side to side.

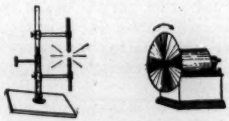

FIG. 12.

In consequence of this it is evident that the light from any alternating arc lamp is in itself a stroboscopic beam, and this is readily seen to be the case. If we throw the beam from this alternate current arc on our rotating discs we see first the same patterns as we did with our rotating disc, but blurred, since the light is not cut off wholly or sharply, as with our rotating disc stroboscope. The patterns can, however, be recognised readily, and this we shall find to be of considerable value later (fig. 12).

If we try the same thing with a glow lamp, we shall find little or no change, unless the filament is very thin, in which case variations of brightness are visible and the lamp shows the patterns also to some extent. If we bring a magnet close to the lamp we see that the filament apparently bends from side to side as the arc did.

Turning to the industrial applications of stroboscopic observation I will mention two or three instances which will show its utility. Whenever we have machinery

rotating at high speed it has a great tendency to cause vibration unless the parts are perfectly balanced.

But it is difficult to do this balancing exactly. The want of balance is only evident when the machine is rotating. A few months ago we had serious trouble from vibration with a high speed motor here, and on considering how to get it balanced, I resolved to try stroboscopic observation. By directing a stroboscopic beam on the end of the shaft while the motor was vibrating, we were distinctly able to see the centre of the shaft apparently moving round the circumference of a small circle, and in two short trials we had the motor perfectly balanced and the vibration stopped, which we might have vainly sought to do for a week without the stroboscope.

It is worth noticing, too, that the balancing was in great part effected by removing the balance weight which the makers had put on to get the static instead of the dynamic balance. Troubles in balancing locomotive wheels, or in any reciprocating machinery, could similarly be observed, or the variations of pressure in an engine cylinder.

Another most valuable stroboscopic application was in the application of the steam turbine to marine purposes. For many years the proper forms of screw propellers has been a subject of controversy, the difficulty being to avoid the screw carrying the water round with it instead of cutting through it. In adapting the screw to the high speed turbine it was found that this difficulty was serious, and Messrs. Parsons solved the problem by actually observing the motion of the water, by running the screw in a tank and observing the currents of water by a stroboscopic beam. It is probably not too much to say that the successful application of the steam turbine to marine work is due to this stroboscopic investigation.

A similar instance came under my notice last year in the United States. There the form of water wheel known as the Pelton wheel is greatly used, and its efficiency greatly depends on the proper shaping of the paddles or buckets that the water strikes against. At the St. Louis Exhibition the General Electric Company exhibited a Pelton wheel which was stroboscopically illuminated, and by means of which the whole course of

the water could be watched, enabling them to choose the form of bucket which gave the least splashing and loss.

We now come upon the ground upon which I have been working for the last few months, namely, the application of the stroboscope to measurements of the speed of rotation or vibration of bodies, or their acceleration, and other useful measurements.

One of the most difficult operations to perform with any degree of accuracy is the measurement of speed of rotation. The number of speed indicators is legion, but I only know of one, and that a very recent one, which is likely to prove reliable and accurate. On account of this difficulty I found it necessary to go rather carefully into the matter, and after devising a special form of indicator, which, though fairly satisfactory, was too delicate for general use, I decided to try stroboscopic measurement.

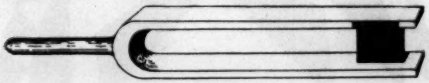

FIG. 13.

If by any means we can obtain a stroboscopic beam of a perfectly definite number of flashes per second, then it is only necessary to attach to the machine under test a diagram with some suitable pattern on it, and observe where this figure appears stationary. In order to obtain this beam I employed a standard tuning fork, making 50 double vibrations per second, and fitted with a shutter on each leg. Slits were made in each of these shutters, and were so arranged as to be opposite one another when the fork was not vibrating (fig. 13).

On setting the fork into vibration and looking through the slits, or projecting a beam through them, we get a series of flashes at the exact rate of 100 per second. Next to a standard clock there is probably no such perfectly regular motion as that of a good tuning fork. Temperature changes affect it, but only to the extent of about one-hundredth of one per cent. per degree Centigrade. Now, if we put on the motor a disc with, say, four dots or arms, and revolve it, those dots will appear stationary when the speed of rotation is one-fourth of the speed of the flashes.

The flashes are at the rate of 100 per second, or 6,000 per minute, so that when the figure is stationary the motor is running at exactly 1,500 revolutions per minute. By putting different rows of different numbers of dots we can get them stationary with various speeds. But on trying this device, which has been used before for the regulation, though not so far, as I am aware, for the measurement of speed, it was found extremely confusing to recognise the different rows.

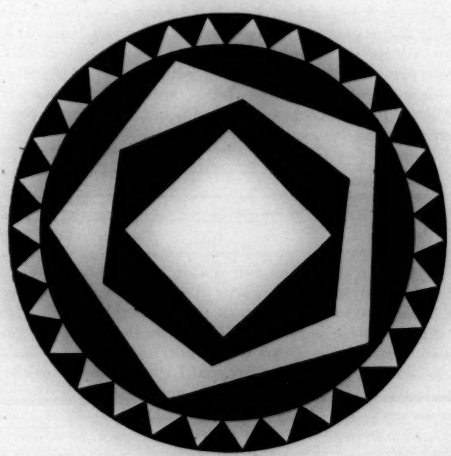

FIG. 14.

It, therefore, struck me that if we made use of some geometrical figures of well known form the mind would at once recognise them. This has proved very successful, and the standard design which we use is shown in the motor here (fig. 14). It consists of a white square inside a black pentagon, which is itself inside a white hexagon, while round the outside of this there is a star or rose of 30 points. A simple calculation shows that the square will appear stationary for speeds of 1,500

and 3,000 revolutions per minute, and will appear as a double square for the intermediate speeds of 750 and 2,250 revolutions.

Similarly the pentagon will appear single for speeds of 1,200, 2,400, and 3,600 revolutions per minute, and double for 600 and 1,800 revolutions. The hexagon will appear single for speeds of 1,000, 2,000, and 3,000 revolutions, and double for 500, 1,500, 2,500, 3,500, etc., revolutions. Other figures can also be recognised. Finally, the 30-point star will appear single for speeds of 200, 400, 600, etc., upwards, and double for 100, 300,

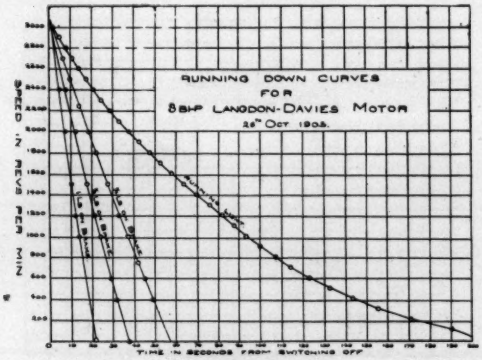

FIG. 15.

500, 700, etc. We can thus readily check any even speed we desire, and this forms an exceedingly accurate and convenient method of checking any ordinary speed indicator.

Again, we can by its means readily see how fast a motor starts and stops. I have here a motor running at a speed of 3,000 revolutions per minute, and as it is switched off you see the single square at 3,000 revolutions, the double hexagon at 2,500, the pentagon at 2,400, the double square at 2,250, the hexagon at 2,000, etc., until it stops. At the same time the rose on the outside becomes stationary every 100 revolutions. I can-

not do better by way of showing you the perfection of this method than to project a slide on the screen, showing how this motor stops, as shown by this method (fig. 15).

Notice the extreme perfection of the observations as shown by the points taken at every 100 revolutions from 3,000 revolutions per minute to stopping. Here is a similar curve taken by some students after a few trials of the method (fig. 16). Here, again, are curves showing how the motor starts up either light or under load (fig. 17). These are of considerable value in comparing various motors.

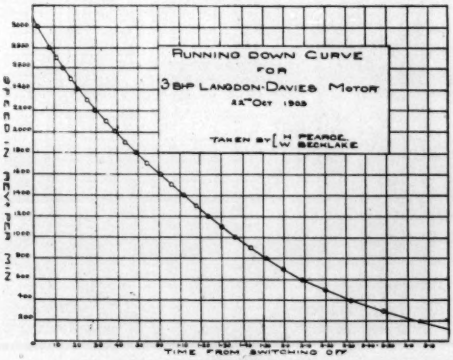

FIG. 16.

But it will be objected that this method does not enable us to measure any speed, it only tells us when a machine is running at some particular number of hundreds of revolutions. I have, therefore, extended the method by employing the tuning fork not to give the stroboscopic beam, but to electrically revolve a conical roller. By looking or projecting the beam through slits in a disc which revolves on the roller, we can measure any speed we desire by traversing the disc along the roller until the figure on the motor appears stationary, and the speed can then be read off on a scale

attached to the apparatus to an accuracy of one part in a thousand.

The same method may be easily adapted to finding the frequency of an alternating current. If the same apparatus is illuminated by an arc lamp or glow lamp from the alternate current supply, we have only to traverse the disc until the figure upon it appears stationary, and the frequency is then shown on the scale.

Again, the same device may be used for another interesting test in connection with motors. I have here a motor which runs from the alternate current supply. Such a motor, if theoretically perfect, *i.e.*, if it had no

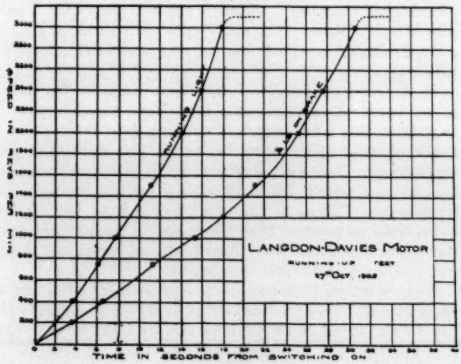

FIG. 17.

friction, would run, when light, at a speed which is an exact fraction of the frequency of the alternate current supply. Owing to the friction of its bearings, etc., it runs at a speed slightly less than this, or it is said to slip. The amount of this slip is immediately seen by throwing a beam from an alternate current arc upon it, and we see the figure of the cross blurred, but rotating steadily.

If we load the motor by putting a brake on it the figure moves faster. If, on the other hand, we send a little direct current into the motor it immediately is locked

in what is termed synchronism, and the cross apparently stands still.

The small roller apparatus gives us an extremely convenient and accurate method of measuring this slip. By running the roller from the motor and the arc off the mains, and traversing the disc until the cross on it appears still, the slip, as a percentage of the speed, is read off directly on the scale.

It is possible to perform many other valuable experiments stroboscopically. For instance, we may measure the angle by which a shaft twists when it is rotating, and use it as a transmission dynamometer, and we can simply measure the difference of phase between two alternating currents. Another application which I employed about three years ago was to the timing of photographic shutters, which can be very accurately done in this way.

In conclusion, though I fear that some of the later experiments on motors, etc., may have wearied you, I hope that they have been not without some interest, and that you will agree with me as to the utility of the stroboscope. I cannot, however, resist one other suggestion as to a possible field of utility for the tuning fork stroboscope. There has been a good deal of feeling for some time past concerning the speed of motor cars and the fines which are sometimes inflicted upon not very exact evidence as to the true speed.

Now, if we only attach to the back shaft of a car a small drum, having a number of spots on it corresponding to the speed limit, and we simply look at that car through slits on a tuning fork we shall see instantly whether it is above or below the limit. If above the spots on the shaft will appear to be moving upwards, if below it they will seem to move downwards. Different rows of dots could be arranged for various speeds.

I have a vision akin to Mr. H. G. Wells' anticipation, when the blue-coated, behelmeted policeman will draw forth the official tuning fork from his pocket as the motor comes tearing by, and after gazing at its back shaft will solemnly inscribe its speed upon the official sheet.

Finally, I must express my great indebtedness to Mr. A. C. Jolley and Mr. Morris, for the experiments you have seen, and to Mr. Dowse for preparing the diagrams.

OPTICAL STANDARDS.

Discussion Introduced by Mr. W. Salt.

The Steps to be taken to bring Optical Standards into General Use.

For the purpose of opening discussion on the subject of the steps to be taken to bring optical standards into general use it does not seem to me necessary to make any very lengthy statement. Perhaps it will be sufficient if I briefly make such suggestions as occur to me and leave it to the meeting to enlarge upon them and shape them into more practical form. This is what I propose to do. It might be well, however, if I offer a brief reminder of what led to the work of standardisation by the Optical Society, and give a short statement of how far that work has already progressed.

No doubt it is remembered by all present that one of the objects of the Optical Society is, and has been all along,

> "To promote the establishment, adoption, and recognition of standard sizes, gauges, calibres, templets, and methods for universal use in the Optical trade."

Very soon after the foundation of the Society—on December 21, 1899, to be precise—Mr. Lionel Laurance contributed an important paper on "Optical Standards," which particularly dealt with standardisation in relation to spectacles and eyegalsses. A little later the Optical Standards Committee was, by direction of the Society, appointed by the Council, and almost immediately met and appointed sub-committees to consider and report upon standardisation in the various directions of the wide range of optics.

The Society had already recommended that standardisation of spectacles and eyeglasses should be dealt with as early as possible, by which it will be noted that this branch of the work of the Optical Standards Committee was considered to be of urgent importance. Accord-

ingly the sub-committee appointed to operate in this direction (this sub-committee, by the way, consisted of members of all sections of the spectacle industry) proceeded with much energy, and, after deliberating no fewer than 24 times, presented their report to the Optical Standards Committee, who laid it before the Society on May 16, 1904. The recommendations in the report then presented were exhaustively discussed, and, after some slight modification, adopted by the Society, and in due course published; but, notwithstanding the fact that a period of 18 months or so has elapsed, it does not appear that the standards adopted and recommended by the Society have become generally employed in the optical trade. The object of the present meeting is to consider what can be done to bring these standards into general use.

In the paper to which I have already made reference, Mr. Laurance stated that—

"The utility of standards in all industries has long been recognised, and they are no less necessary in the optical than in any other trade, whether they be standards of measurement, of size, of quality, or of design."

I believe we can all agree with this statement, and, furthermore, I think all will admit that for standards to be of real utility they must be not only carefully and intelligently devised, but consistently and widely used. That the standards now before us are needful in the industry has been decided by the Society already; it is now for us to try to arrange that they shall be brought into universal use.

Not only were these standards published in the optical press, but on June 16, 1904, your Secretary forwarded to a very large number of Opticians in all parts of the country a printed copy of the recommendations of the Society with an invitation to traders to assist in making the standards generally accepted and used. That invitation did not meet with much direct response, which, I think, was mainly because dealers in various parts, or, I might say, because persons interested, waited to see what would be done in the matter by manufacturing firms. It may be reasonable for the ordinary trader to look to the manufacturers to make the first movement towards popularising a system of standards, but at the same time the willingness of the dealer to recog-

nise and work according to that system is necessary to be assured.

Obviously it would be a very unfortunate thing if the great work of the Optical Standards Committee and its sub-committee were not taken full advantage of, especially when it is true that the results were arrived at after most exhaustive research and painstaking examination. For my part I believe this is very generally recognised, and I am of opinion that once a vigorous start to work, according to the adopted standards, is made it will not be long before the system commands attention far and wide.

As I have said, it is perhaps desirable that I should make some tentative proposals, leaving them open for your discussion. My first suggestion, then, is, that manufacturing firms should be definitely invited by the Society to construct their articles in accordance with these standards, and in turn invite their customers to recognise such slight changes as may thereby be brought about, and order their goods accordingly.

My second suggestion is that members of the Optical Society and of all other optical societies and organisations, should be asked to pledge themselves to adopt the standards and use them regularly.

The third proposal I will make is that as far as possible foreign manufacturers who send their work to this country be informed that the new standards will henceforth be used by dealers here. I think it will be found that such manufacturers will not be slow to shape their plans accordingly. Notwithstanding what may be thought or said to the contrary, I think it is evident that we have not yet found it convenient, if possible, to ignore the productions of other countries in the way of optical instruments, and this is why I desire to urge that our foreign friends should be taken into our confidence to this extent, which will be fully as much to our benefit as to theirs.

Here I might be allowed to remark that the Optical Society's system of standards so far involves no change of a serious nature. It is true that for some time past there has existed, and been very extensively used, in England a series of sizes of eyes of spectacles and eyeglasses. Very likely it is felt that the Society's sizes of eyes differ from those to which very many traders have

now become well accustomed. But that is not the case.

I am able to say that the sub-committee, in dealing with this part of their work, realised that if those already established sizes were ignored, and fresh ones substituted, much confusion, if not great displeasure, would result. It should be made clearly known that although in the Society's list of standard size of eyes the numbering differs from the already established set, the actual sizes are the same, except, I believe, in the case of the largest size of all, which, as every one knows, is very seldom used at all for ordinary spectacles and eyeglasses. The reason for this slight change lies in the fact that by making it the Society's series was graded in a systematic order which is easily learnt and remembered.

Another proposal I will make is, that means be taken to persuade all surgeon oculists to use only the system of angle notation for cylindrical lenses, etc., which is recommended by the Society. From experience and from observation I think I can safely say that if this system were universal a very great deal of annoyance, delay, and even injury, might be averted.

The general arrangement of the Society's set of standards is, no doubt, so well known to you that I need not go further into detail at this moment. You will be aware that some of the methods recommended have for long been very commonly used, and you will also know that the whole of the recommendations of the Optical Standards Committee were fully discussed by the Society before their adoption was recorded.

I would like to go further into detail, and, in fact, to take the recommendations one by one and remark upon them, but this would weary you and probably serve no useful purpose, especially as I believe a copy is in the hands of most of those present. Besides, I have no doubt that before now every member of the Society has carefully studied them. Therefore, having now made my several suggestions I leave the matter to your discussion. In doing this I am filled with hope that what you decide to-night will bring about in the near future the general adoption of the Society's standards, not only in England, but in all places beyond the seas, to the great convenience and the general advantage of all concerned.

Dr. WALMSLEY said that he rose in obedience to the Chairman's command to continue the discussion, though personally he would have preferred to have heard something from the trade point of view before being called upon to intervene. Certain points had occurred to him while Mr. Salt had been speaking, and some other points had been in his mind before he came to the meeting; but before referring to any of these he should like to emphasize the point (lest it should be misunderstood outside) that the particular section of standards which they were now discussing only represented a part of the work of the Optical Standards Committee, referring, as it did, to spectacles and eyeglasses only.

The Optical Standards Committee consisted of a number of very busy men, and it was exceedingly difficult for them to devote as much time as they would like to the consideration of the various divisions of the subject; the consequence was that there certainly was some force in a remark which had fallen from Mr. Salt, that at present the work of that Committee "appeared to be sleeping." However, several sub-committees were quietly at work considering other branches of the subject, and in due course the results of their deliberations would be made known.

But, as Mr. Salt had remarked, the question of the standardisation of spectacles and eyeglasses was essentially an urgent and pressing matter, which the members of the Standards Committee considered fully ripe for settlement—so much so, that they had reported to the Society, which had, after full discussion, adopted their recommendations, and those recommendations were now before the world as the considered opinion of that Society. The question of the moment was, how they were to get those recommendations generally adopted. Mr. Salt had recited the steps taken so far, and, since those steps had not resulted in a very wide adoption of the proposed standards, notwithstanding that they had been adopted by the Society so long ago as May 16th, 1904, it was certainly time that they should consider very carefully, and with all the light that their united experience could throw upon the question, what further steps might be taken to secure the end in view.

There were three separate classes to be considered

in relation to this question of standardising spectacles and eyeglasses, viz., the manufacturers, the traders, and the oculists, and unless they could get the standards adopted by all three classes, then, to the extent that they were not adopted, the labours of the Standards Committee would have been wasted. He would like to remind the Society that that Committee had not made those recommendations without what, he thought he was justified in saying, was an enormous amount of labour. They were not made at a single meeting round a table; there were many meetings, many letters were written, all kinds of parties were consulted, and their utmost endeavours had been to obtain such a series of standards as would commend themselves to the trade as being based upon sound scientific principles and existing practice.

His own personal belief was that they had such a set of standards in the Report which was in the hands of those present, and what they now wanted was some practical suggestions in the direction of securing the general adoption of those standards. Mr. Salt had made one or two such suggestions, and he would venture to make a further one, viz., that every trade member of that Society, whether on the traders' side or on the manufacturing side, should print prominently at the top of his notepaper, invoices, and so forth, some such words as these:—"The Optical Society's Standards and Nomenclature Used."

Many of the terms used in the Committee's Report were at present employed somewhat vaguely, and it would certainly be an immense advantage if the person employing such terms knew that in all cases the recipient of his letter, or order, would attach to those terms the definite meanings laid down in the Report. He need hardly point out that the adoption of that simple suggestion would involve no expense whatever beyond the initial cost of setting up the type or cutting the plate, and if even a small number of firms took up the matter in this way, the light would gradually spread in ever-widening circles, and they could not tell how far it might reach. It would certainly reach those enterprising Continental manufacturers who, rightly or wrongly, were credited with always finding out what their customers wanted, and taking measures

accordingly. He was, of course, not wedded to any particular form of words, but he thought it important that some uniform phrase should be employed, and that the form of words should not be left to mere individual caprice.

It was not only necessary that the form of words, whatever it might be, should be printed upon the note-paper, invoices, etc., but that there should be at hand for reference by those who read the words, or easily procurable, a copy of the recommendations of the Standards Committee, and while upon that point he might say that if it were necessary to reprint that document, in order to carry out his suggestion, it would be desirable to adopt some more convenient size of paper than the foolscap size, which was not very suitable either for the desk or for the pocket.

He thought Mr. Salt's suggestion that the various scientific societies should be urged to adopt the standards was an extremely valuable one, and he would suggest that they should, by means of a resolution which their Secretary could quote in his correspondence, formally request those societies to adopt the standards for all optical work, of course, accompanying the suggestion with a few copies of the document in which the standards were embodied, and with an intimation that further copies could be obtained if desired. He did not propose to discuss the standards themselves at all; those, he assumed, were past the discussion stage now, and that the meeting must assume that these were the best standards that could be devised.

Mr. DIXEY said the form in which the discussion had been announced apparently precluded the possibility of considering the desirability of the standards as recommended by the Committee, although a good deal might doubtless be said about that. They were asked to discuss the question of the best means of securing the adoption of these standards by the optical trade, and from that he thought it was fair to infer that they had not been generally accepted. If that were so, it would certainly seem pertinent to enquire into the reason of their failure to secure general acceptance. He would like to point out that the recommendations contained in the paper of Optical Society Standards before them were extremely varied in character, and it

was easy to pick out some which it would be very simple to adopt, and others the general adoption of which was quite impossible.

He spoke as one representing the class which would perhaps be most affected by these recommendations—namely, as a retail Optician—and from that point of view he desired to remind them that optical standards referring to spectacles and eyeglasses were not exactly on the same footing as standards in the engineering trade, or in the optical instrument trade. There was this essential difference—that spectacles ought not to be made in gross; any well-made pair of spectacles or eyeglasses ought to be separately finished, as to a certain extent it was a work of art, and the workman must take a pride in it if it was to be worth selling and worth wearing. Of course, this remark did not apply to all the recommendations, but only to some of them, and for his part he thought it should be left to the discretion of the individual trader (he was still speaking as a retail Optician) which he adopted and which he did not adopt.

It was common knowledge that many traders preferred their own standards, and quite justifiably so. Some of the standards he should certainly not adopt himself, because they would involve so many sacrifices, but on the other hand some of them ought certainly to be adopted by everybody, and he was quite of opinion that some steps should be taken to that end.

Mr. Dixey, after illustrating his remarks by referring at considerable length to the various recommendations of the Committee, went on to say that he did not altogether like any of the suggestions made either by Mr. Salt or by Dr. Walmsley; none of them struck him as being very useful, and, on the whole, he thought it was better to leave it to individual influence to gradually make its way. He thought the universal adoption of all the recommendations was a thing which would never come to pass, and which, indeed, was not at all desirable.

In conclusion, he might say, as a member of the Standards Committee, that the recommendations did not in all cases represent the unanimous opinion of all the members; there was a great deal of time spent in

discussing them, and sometimes they represented the opinion of the majority only.

Mr. PURSER said that the larger part of the objections raised by Mr. Dixey were very old news, and he thought it was much better that they should all frankly accept these standards rather than engage in a futile fight against a reform which was bound ultimately to come. Still, there were one or two things which it was necessary for them to do before they could get these standards generally adopted. The recommendations, as printed, were no doubt very useful to the Optical Society itself, as a scientific institution, and also to the manufacturers, but they were possibly not so useful to the ordinary Optician. For instance, taking the question of coloured glasses, it was useless giving the Optician a list informing him that No. 1 transmitted 80 per cent. of incident white light, because he could not measure it, having no apparatus for the purpose.

Therefore, some supplemental information ought to be furnished to the retail Optician on this point. His suggestion was that the approximate tint should be placed on record on the sheet, and by some of the wholesale houses this was already done. Then, as regards the width of eye-wires, not one in a hundred of the retail Opticians would know what No. 20 (6s. wire) really was, and it would be very useful if they could include in their list of recommendations a diagram illustrating the actual gauges of the wires. Diagrams illustrating the spread of bridges might also be given, thereby encouraging the busy Optician to make daily use of the standards system in ordering his goods.

Then, as to the adoption of the angle notation, the real difficulty there would be to get the oculists to change the system to which they had been accustomed for a great number of years. He would therefore propose the addition of two or three items to the list of recommendations, giving particulars such as those to which he had referred, and he was sure that then the movement would meet with far more support from the great majority of Opticians in the country.

Mr. A. E. GREY said that, having arrived late he had not had the advantage of listening to the suggestions made by Mr. Salt, and had only heard the latter portion of Mr. Dixey's criticisms. He himself supported all

the recommendations of the Committee, and as Mr. Dixey was for some time a member of the Committee, he was rather surprised to find him now raising objections to some of the results of the work in which he had taken part. From the point of view of utility, and from the point of view of supplying better work to the community, standardisation was in these days, in his opinion, a matter of necessity.

It had been recognised in other trades equally scientific and artistic with their own, and in every such case standards, when adopted, had been found to all concerned beneficial. It was most important that every effort should be made by that Society to secure the general adoption of the standards upon which it had decided. Take the case of angle notation. Was it a good thing that the present state of things should continue to exist—two or three systems in use? Was it not better that they should by every means in their power endeavour to secure the adoption of a single system? And so with all the other branches of the question.

As to the particular means of obtaining the uniformity at which they aimed, he thought for one thing they should approach in person societies like the Ophthalmological Society, and point out to them the usefulness of their adopting these standards. They ought to study the general convenience of the people who had to manufacture these goods, and to help each other by rendering their proper manufacture as easy and as simple as possible. He believed if the matter were properly brought before the ophthalmic surgeons they would fall in with the general wishes and desires of the trade.

As a manufacturer, he considered it was highly desirable, and, indeed, absolutely necessary, that they should adopt these standards, and that such methods should be brought into use as would be for the common good, and as would help in the production of these articles in the best and cheapest manner. Mr. Dixey had told them plainly that he objected to the standardising of the sizes of eyes, because he chose to make his own sizes; but there were many people in this country who could not afford to pay the price which could be commanded by a gentleman like Mr. Dixey, and if they standardised the sizes of eyes they would then be in a

position to supply a good article to poor people at a low price. He believed there was a demand among the members of the trade to have these standards brought into general use, and, in his opinion, it was the bounden duty of the Society to do all in its power to bring about such a state of things.

Mr. SELBY said that as regards the paragraph headed "Trial Cases" on the last page of the Recommendations, where it was stated that it was "desirable for trial cases, ophthalmoscopes, and other instruments used for sight testing purposes, to be certificated by the National Physical Laboratory," it might, perhaps, be of some interest to the Society to know what was the present position in regard to such certification. As many members of the Society were aware, the Laboratory had been for some time occupied in making arrangements for testing trial cases, and he was happy to state that at the present moment that work was practically completed—(Hear, hear)—and it was desirable that as soon as possible a meeting of the sub-committee should be held in order to finally fix and determine upon the limits of error which should be allowed in the glasses contained in trial cases. As soon as that was done the National Physical Laboratory would be ready to undertake the testing of trial cases, and he hoped that very shortly they would be having such cases sent in for testing.

That was not the occasion to go into any detail, but in connection with the subject of tinted glasses, to which reference had been made, some observations were being made at the Laboratory, with a view to arranging for the testing of the coloured glasses included in trial cases, and possibly they might be able to help in some degree in settling the question of the standards of colours which were set out in paragraph (4). He would like to mention also, although he was wandering somewhat far from the matter which had formed the principal subject of discussion, one point which arose in connection with the work of testing trial cases, and that was the difficulty of marking them as certified by the Laboratory.

It was essential that the lenses should be marked after they were tested, and it was desirable that, if possible, the mark should be on the lens itself, and not on the

mount. He only referred to the matter to-night in the hope that members might, perhaps, be able to offer some suggestions as to the most rapid and convenient way of marking the glasses for the purpose of identification.

Dr. WALMSLEY craved the indulgence of the meeting in order to make one further remark. Mr. Dixey had said that the adoption of certain of these recommendations, more particularly angle notation, might lead to certain expense to some ophthalmic surgeons. Well, of course the adoption of any system of standardisation which replaced other systems in use must involve expense to individuals; but it had been done over and over again. As an example, he might instance the Engineering Standards' Committee which recommended certain standards for joints and things of that sort. Possibly some of the members did not realise what it meant to alter the standards of a rolling mill. A rolling mill was a very costly piece of machinery, running into thousands of pounds, and the rolls through which the iron had to pass were a very important part of the stock in trade of each one of these great works. Yet the manufacturers were ready to undertake the great expense of constructing new rolls. They could not make great changes of this sort without some individual sacrifices, but if the trade generally was convinced that the benefits to be derived from the changes proposed were sufficient to justify those sacrifices he thought the sacrifices ought to be, and would be, made.

Mr. DIXEY said it was not only the difficulty of altering the instruments, but of altering a man's method. A surgeon who had performed an operation in a particular way for 20 or 30 years was to be asked to change his method of conducting the operation, not for any essential advantage, but that his tools and implements might be adopted to a universal standard. If Dr. Walmsley would undertake the work of visiting the ophthalmic-surgeons and suggesting these changes, he would be pleased to furnish him with a list of names, but he confessed he did not envy him the task.

The CHAIRMAN said he wished to say a few words before calling upon Mr. Salt to reply to his critics. In the first place, he would like to tell Dr. Walmsley, who had naturally resented the fact that he was the first speaker called upon, that he had taken that course for

the very reason that he (Dr. Walmsley) was not a practical member of the industry, and he had felt sure, and his assurance had been justified, that he would approach the question from the economic point of view, which would be very valuable to them. Mr. Dixey had told them that for artistic reasons, which he had explained, he would not be prepared to adopt the whole of these standards. That meant to say that an artist in spectacles was such an incapable artist that he could not produce two similar works of art on similar lines.

So far as he knew the work of spectacle making, that was a condition of things which did not prevail; it certainly did not prevail with regard to other art efforts in technical commercial life. But the point was this: the public could not all go to Bond Street for their work; they might walk from Bond Street to Bethnal Green, where incomes were not so great, although eyes might be as beautiful, and as much in need of treatment as in the case of those who promenaded Bond Street. And it was obvious, even to himself, that there must be considerable economy entailed in the workshop and elsewhere by using these standards.

Let them consider who were the members of the trade who had worked on their standard committees, and if anybody would take the trouble to inquire of the honorary secretary as to the debates at the various meetings, he would find that the consensus of opinion was almost perfect, and that there was little or no discord over any of the recommendations which appeared in the Standards list before them. Now, recommendations emanating from such very practical men must surely be of value to the industry, and could not be productive of inartistic work in the way which Mr. Dixey appeared to fear.

Dr. Walmsley had mentioned the engineering industry, but he would refer to an industry much nearer home, viz., the photographic industry. They all knew that standards were introduced into the photographic industry some years ago, and he would ask them, had the work of the photographic manufacturing industry in any way depreciated? As a matter of fact, photographic work became more useful and better finished year by year, and if they went into any workshop where photographic commodities were turned out, they would

be told that the work was rendered through standardisation infinitely easier than ever before.

Not only so, but these comparatively facile methods of manufacture enabled them to keep prices at such a level that competition was comparatively easily faced.

With regard to Mr. Purser's suggestion that the Optical Society should go to the expense of giving practical illustrations of what these optical standards denoted, it appeared to him that if they were going to do that, they need not have the nomenclature of optical standards at all. He took it that those standards were supposed to be an easy method of expressing what was required, but at the same time it occurred to him that there was money to be made by some practical Optician who chose to take the trouble of bringing out and publishing a text book of samples representing the Optical Society's Standards for the use of those beginning to learn their trade.

He regarded Mr. Salt's suggestions for securing the adoption of the recommendations as very practical and very useful, and he also regarded with favour the idea put forward by Mr. Purser, of calling a meeting of those interested in the trade, in order to discuss the matter of adopting these standards.

As to the oculists, he thought they might be got hold of in a more simple way than had been proposed to-night. The forms on which they were accustomed to write their prescriptions were, he believed, although he was open to correction, usually supplied to them by the Opticians to whom they sent their patients for glasses, and it would be easy for those Opticians to insert into their forms such points as were requisite in order to constrain those who used them to employ the standards.

Mr. DIXEY said that so far as his experience went it was not the general custom for oculists to accept their diagrams from the Opticians with whom they traded—he did not believe it was done in more than one case out of ten. Before sitting down, he would like to ask Mr. Salt to give, in his reply, the reason, in his opinion, why these recommendations had not been generally accepted by the retail Opticians.

Mr. SALT in replying on the whole discussion, said that at the outset he would answer, as well as he could, the question just put by Mr. Dixey. In his opinion one

very great cause of the non-adoption of the recommendations by retailers was simply because all were waiting for some one else to make a start, and that was the reason why he had made his first suggestion—that the manufacturers should be invited in some way or other to make up their work in accordance with the Optical Society's Standards, and to let their customers know that they were using, and working according to, those standards, and to request them to co-operate with them in the work.

One house with which he was connected had, in fact, taken up the matter to some extent, but, of course, one house could not do everything, and they must try and make the movement as wide and general as possible if it was to be really successful. Mr. Salt proceeded to recapitulate (for the benefit of Mr. Grey, who had not previously heard them) the various suggestions he had made in his opening address, and went on to say that he would like to remind certain speakers, who appeared to have forgotten the fact, that these standards were not adopted merely by a sub-committee, or merely by the Standards Committee, but were adopted by the Optical Society itself.

Moreover, they were adopted, in a very large sense, by the trade throughout the country, for the simple reason that the trade generally was invited, both by circular and through the trade journals, to give its opinion on the proposed standards, and as it had not made any adverse criticisms upon such standards, it was a fair assumption that the trade was in favour of them. Of course, there might be exceptions, but generally speaking, the fact was as he had stated. He expressed much gratification at the remarks made by Mr. Selby with reference to the standardisation and certification of trial cases by the National Physical Laboratory, and he felt very strongly that this was an even more vitally important matter in many ways than the standardisation of spectacles and eyeglasses.

As Dr. Walmsley and the Chairman had, by their remarks, largely anticipated what he had intended to say in his reply, there was really not a great deal left for him to add. He considered that Dr. Walmsley's suggestion, that manufacturers should adopt some form of words in their note headings and invoices, signifying

their adoption of the standards, was an exceedingly valuable one. With regard to Mr. Dixey's criticisms, Mr. Dixey was one who, undoubtedly, knew a very great deal about spectacles and eyeglasses, but he was afraid that he looked at the whole matter from a too narrow standpoint.

Mr. Dixey had said that the making of a pair of spectacles was a work of art, and so it was; but surely it could not be said that that work of art would be spoilt by, for example, having the eyes beautifully symmetrical, which could so easily be done by the use of an interchangeable system of eyes. He thought it would be a great pity if they should allow the discussion to be a barren one, and did not come to some definite resolution before parting, and as no one had as yet put any resolution before the meeting, he would venture himself to move (though he would have preferred the motion to come from the body of the meeting) "That a letter be officially sent from the Optical Society inviting the various manufacturing firms to make their work in accordance with the standards adopted by the Optical Society, and asking them to request their customers to co-operate with them accordingly."

Mr. PURSER seconded the motion.

Mr. GREY said he would have preferred to see one of the main difficulties attacked before sending round to the wholesale houses, namely, the difficulty with regard to the medical side of the subject. One of the first things to be done, in his opinion, was for this Society to officially approach the Ophthalmic Society, and get them to agree to these standards, and he did not believe there would be much difficulty in getting their assent. Then with regard to the manufacturers, would it not be better to call a meeting of all the manufacturers, at which the whole question could be discussed under the auspices of the Optical Society? An official letter, such as Mr. Salt had suggested, was only too likely to be thrown into the fire, and a meeting would, in his opinion, have far better results.

Mr. SALT said that he had no objection to withdrawing his resolution and consenting to the substitution of one framed in accordance with Mr. Grey's suggestion. Mr. Purser agreed also to this course, and the resolution was accordingly withdrawn.

Mr. A. E. GREY then moved:—"That this Society call a meeting of the manufacturers interested in the Optical Society's standards, for the purpose of considering the steps to be taken to bring such standards into general use."

Mr. PURSER: By whom will the meeting be convened?

The CHAIRMAN: By the Secretary, naturally.

Mr. DIXEY: Are the Birmingham and Wolverhampton makers to be invited?

Mr. A. E. GREY: Certainly—all the manufacturers in the country. Unless the standards are adopted universally throughout the country, the work of the Standards Committee is useless.

The CHAIRMAN said he thought they might go one step further with regard to their own members, and he would venture to put a resolution which had occurred to him while listening to Mr. Salt's opening remarks, viz., "That members of the Optical Society be individually invited to pledge themselves to support the Optical Standards." Mr. A. E. Grey seconded the resolution. Mr. Dixey wished the resolution could be made a little more definite.

The CHAIRMAN said he did not well see how that could be done. A circular would be sent round to all the members, and they would answer either in the affirmative or in the negative, or, in certain cases, as what he might call "half and half" supporters. The resolution was then put and carried.

A NEW SPECTROMETER—ITS USES AND ADVANTAGES.

By Val. H. Mackinney.

Read before the Optical Society on February 15th, 1906.

In presenting my paper to you this evening allow me to thank you heartily for the great honour which you have bestowed upon me, by asking me personally to lay the results of my investigations before you. Except for one brief remark, in conclusion, it is my intention to limit the scope of this paper to the spectroscopic examination of glass, illustrating thereby the advantages offered by the new spectrometer—to my own design—which you see before you this evening.

I must, however, mention that the instrument before you does not exhibit all the advantages mentioned in my paper, it being but an old base to which new arrangements have been fitted, to illustrate the main features of the new design. Nevertheless, it has served me well for experimental purposes, and I may say, that it depends on how this paper is received as to whether an instrument is constructed exclusively to my design or not.

Theory.

Part I.—Index and Dispersion.

The angles of incidence and refraction always lie in the same plane with the normal to the surface, and their sines are to one another in an invariable ratio.

This is the fundamental law of refraction, and the constant ratio depends on the kind of light transmitted and the nature of the two media. Calling the medium in which the light is incident the "incident medium" and the medium in which the light is refracted the "refractive medium," we have it that the constant ratio is called the *refractive index*, from the incident to the refractive medium.

For specifying the refractive properties of a glass Abbé used the following five bright spectral lines:— The red of potassium, the yellow of sodium, and the hydrogen lines $H\alpha$, $H\beta$, and $H\gamma$, since they are always readily obtained from artificial sources. The lines $H\alpha$, $H\beta$, and the yellow of sodium are identical with the Fraunhofer lines C, F, and D, in the solar spectrum, respectively; while the red line of potassium lies near Fraunhofer's line A, and the $H\gamma$ line of hydrogen near

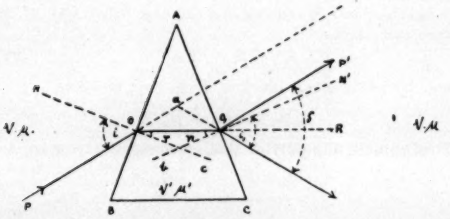

FIG. 1

his G^1 line of the solar spectrum. Abbé, therefore, denoted his lines as follows:—

			Wave lengths.
Red line of Potassium	... A′	...	0.7677. μ
Alpha line of Hydrogen	... C	...	0.6563. μ
Yellow line of Sodium	... D	...	0.5893. μ
Beta line of Hydrogen	... F	...	0.4862. μ
Gamma line of Hydrogen	... G′	...	0.4341. μ

(μ denotes one micron, *i.e.*, .001 mm.)

In the case of A′ and D, which are double lines, the mean is taken.

The fact that the constituents of white light may be separated out and demonstrated, is called *dispersion*, and in order to seek a proper measure of the dispersive power of a substance we must select some well defined line in the spectrum as representing the standard ray. The mean of the two sodium lines has been taken as representing this ray, and its refractive index is always denoted by μ_D. The quantity denoting the dispersive

[1] See Hovestadt's Jena Glass, Chap. II.

power of a substance must be independent of the refracting angle of the prism used in the experiment, hence the refracting angle must be eliminated from the equation which is to represent the dispersive power.

We proceed as follows:—

When the refracting angle of the prism (A) is small

$$\delta = (\mu_D - 1)\, A \qquad (1)$$

where δ represents the minimum deviation for the standard ray.

Let μ_x and δ_x correspond to any other ray of the spectrum, then

$$\delta_x = (\mu_x - 1)\, A \qquad (2)$$

and by subtracting the first from this we have,

$$\delta_x - \delta = (\mu_x - \mu_D)\, A$$

To eliminate A we divide this by equation (1), then,

$$\frac{\delta_x - \delta}{\delta} = \frac{\mu_x - \mu_D}{\mu_D - 1} \qquad (3)$$

This quantity $\dfrac{\mu_x - \mu_D}{\mu_D - 1}$ represents what is known as the dispersive power of the glass, except that in practice the numerator becomes $\mu - \mu_C$ (usually represented by $\triangle$), *i.e.*, what is termed the mean dispersion of the glass.

The final form of equation (3), representing the dispersive power by ω is, therefore,

$$\omega = \frac{\triangle}{\mu_D - 1} \qquad (4)$$

In the spectroscopic examination of glass it is seen, therefore, that to determine the *mean refractive index*, the *mean dispersion*, and the *dispersive power*, one has only to determine the refractive indices of the glass for the D, F, and C lines. It is, however, of extreme importance that the method employed should produce accurate results. For instance, enable the refractive indices to be obtained to four, and the refractive differences to five decimal places.

There are many methods of determining the refractive indices, but I think not one instrument, the use of which will enable any one of four methods to be employed—and which provides such facilities for checking results—as the one shown in (fig. 5) the accompanying photograph, and which you see before you this evening.

It will, perhaps, be advisable here to illustrate the properties of thick prisms more fully before describing the advantages offered by the new instrument.

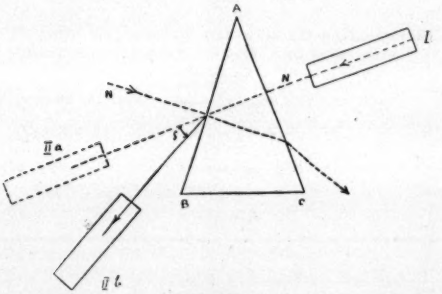

FIG. 2.

Part II.—The Properties of Thick Prisms.

Def. An optical prism is a transparent solid, two of whose faces, at least, are plane surfaces which intersect in a line.

From fig. 1 it will be seen that the following relations exist:—

$$\begin{aligned} \text{P'O'Q} &= \text{P'O'R} + \text{RO'Q} = \delta \\ \text{P'O'R} &= a\,\text{OO'} = i - r \\ \text{RO'Q} &= i' - r' \\ \text{Hence} \quad \delta &= i + i' - (r + r') \\ \text{but} \quad c\,b\,\text{O'} &= r + r' = \text{A} \\ \therefore \quad \delta &= i + i' - \text{A}. \end{aligned}$$

Now, the ratio of the velocity of light in the surrounding medium to its velocity in the prism under consideration, represents the refractive index of that prism.

Hence, from fig. 1 we have,

$\mu' = \frac{V}{V'} = \frac{\mu'}{\mu}$ (if μ of surrounding medium be taken as unity).

Further, this ratio is equal to that of the sines of the angles of incidence and refraction. Therefore,

$$\mu' = \frac{V}{V'} = \frac{\text{Sin } i}{\text{Sin } r} = \frac{\text{Sin } i'}{\text{Sin } r'}.$$

Representing the refractive index of the prism by μ, the fundamental equations for the prism are, therefore,

$$\mu = \frac{\text{Sin } i}{\text{Sin } r} \tag{1}$$

$$\mu' = \frac{\text{Sin } i'}{\text{Sin } r'} \tag{2}$$

$$A = r + r' \tag{3}$$

$$\delta = i + i' - A. \tag{4}$$

In practice we require a general equation which shall connect the index of refraction with the angles A, i, and i', and to obtain this we proceed as follows:—

From the above equations we have,

Sin $i' = \mu$ Sin $r' = \mu$ Sin (A $- r$) which by expanding Sin (A $- r$) becomes,

$$\text{Sin } i' = \mu (\text{Sin A Cos } r - \text{Cos A Sin } r).$$

$$\text{Now Sin } r = \frac{\text{Sin } i}{\mu}$$

$$\text{And Cos } r = \sqrt{1 - \text{Sin}^2 r}$$

$$= \sqrt{1 - \frac{\text{Sin}^2 i}{\mu^2}}$$

$$= \sqrt{\frac{\mu^2 - \text{Sin}^2 i}{\mu^2}}$$

$$= \frac{1}{\mu} \sqrt{\mu^2 - \text{Sin}^2 i}.$$

These values introduced into the above equation will reduce it to,

$$\text{Sin } i' = \text{Sin A } \sqrt{\mu^2 - \text{Sin}^2 i} - \text{Cos A Sin } i \qquad (5)$$

This equation holds good without any conditions being imposed upon the quantities involved whatsoever. We had better, however, solve this equation for μ^2, since in practice it is the refractive index we most frequently require to know. Solving for μ^2 equation (5) becomes,

$$\text{Cos A Sin } i + \text{Sin } i' = \text{Sin A } \sqrt{\mu^2 - \text{Sin}^2 i}$$

Squaring both sides we have,

$$\text{Cos}^2\text{A Sin}^2 i + \text{Sin}^2 i' + 2 \text{ Cos A Sin } i \text{ Sin } i' = \text{Sin}^2\text{A } \mu^2 - \text{Sin}^2\text{A Sin}^2 i.$$

Therefore, we have,

$$\mu^2 = \frac{\text{Cos}^2\text{A Sin}^2 i + \text{Sin}^2 i' + 2 \text{ Cos A Sin } i \text{ Sin } i' + \text{Sin}^2\text{A Sin}^2 i}{\text{Sin}^2\text{A}}$$

$$= \frac{\text{Sin}^2 i(\text{Cos}^2\text{A} + \text{Sin}^2\text{A}) + \text{Sin}^2 i' + 2 \text{ Cos A Sin } i \text{ Sin } i'}{\text{Sin}^2\text{A}}$$

But $\text{Cos}^2\text{A} + \text{Sin}^2\text{A} = 1$, therefore,

$$\mu^2 = \frac{\text{Sin}^2 i + \text{Sin}^2 i' + 2 \text{ Cos A Sin } i \text{ Sin } i'}{\text{Sin}^2\text{A}} \qquad (6)$$

This equation (6) is extremely cumbersome, and may be considerably reduced, providing the necessary data are obtained under special circumstances. There are altogether three special cases in which the above equation admits of still further simplification—they are as follows:—

Case I. (fig. 2).—When the emergent path of the wave of light is normal to the surface, in which case $\text{Sin } i' = 0$.

Case II. (fig. 3).—When the emergent path of the wave of light makes an angle with its normal equal to the original angle of incidence, in which case the deviation is a minimum (see App. C.) and $\text{Sin } i = \text{Sin } i'$.

Case III. (fig. 4).—When the emergent path of the wave of light is coincident with the surface of the prism, in which case r' is at the critical angle and $\text{Sin } i' = 1$.

FOR CASE I.—When Sin $i' = 0$, it will be seen that equation (6) becomes at once,

$$\mu = \frac{\text{Sin } i}{\text{Sin A}}$$

$$\therefore \quad \mu = \frac{\text{Sin } (A+\delta)}{\text{Sin A}} \qquad (7)$$

FOR CASE II.—When Sin $i' =$ Sin i, the simplification of equation (6) proceeds as follows:—

We have directly that

$$\mu^2 = \frac{2 \text{ Sin}^2 i + 2 \text{ Cos A Sin}^2 i}{\text{Sin}^2 A}$$

$$= \frac{2 \text{ Sin}^2 i \ (1 + \text{Cos A})}{\text{Sin}^2 A} \text{ which by expanding}$$

$$= \frac{2 \text{ Sin}^2 i \left(1 + \text{Cos}^2 \frac{A}{2} - \text{Sin}^2 \frac{A}{2}\right)}{4 \text{ Sin}^2 \frac{A}{2} . \text{Cos}^2 \frac{A}{2}}$$

But $1 - \text{Sin}^2 \frac{A}{2} = \text{Cos}^2 \frac{A}{2}$, therefore,

$$\mu^2 = \frac{4 \text{ Sin}^2 i . \text{Cos}^2 \frac{A}{2}}{4 \text{ Sin}^2 \frac{A}{2} . \text{Cos}^2 \frac{A}{2}}$$

$$= \frac{\text{Sin}^2 i}{\text{Sin}^2 \frac{A}{2}}$$

$$\therefore \mu = \frac{\text{Sin}\left(\frac{A+\delta}{2}\right)}{\text{Sin}\left(\frac{A}{2}\right)} \quad . \qquad (8

FOR CASE III.—When Sin $i' = 1$, it is seen that equation (6) becomes – from its old form shown in (5),

$$1 = \text{Sin A } \sqrt{\mu^2 - \text{Sin}^2 i} - \text{Cos A Sin } i,$$

and from its later form (6) we have—

$$\mu^2 = \frac{\text{Sin}^2 i}{\text{Sin}^2 \text{A}} + \frac{1}{\text{Sin}^2 \text{A}} + \frac{2 \text{ Cos A Sin}}{\text{Sin}^2 \text{A}}$$

Now $\dfrac{1}{\text{Sin}^2 \text{A}} = \text{Cosec}^2 \text{A}$

$$= 1 + \text{Cotan}^2 \text{A}$$

$$= 1 + \frac{\text{Cos}^2 \text{A}}{\text{Sin}^2 \text{A}}, \text{ therefore}$$

$$\mu^2 = 1 + \frac{\text{Sin}^2 i + \text{Cos}^2 \text{A} + 2 \text{ Cos A Sin } i}{\text{Sin}^2 \text{A}}$$

$$\therefore \quad \mu^2 = 1 + \left(\frac{\text{Cos A} + \text{Sin } i}{\text{Sin A}} \right)^2 \qquad (9)$$

To sum up the equations which enable us to determine the μ of glass in some form of prism, we have

I. $\mu = \dfrac{\text{Sin } (\text{A} + \delta)}{\text{Sin A}}$. This equation requires the emergent path of the wave of light (or, of course, the incident path) to be normal to the surface, while the data are being determined (see fig. 2).

II. $\mu = \dfrac{\text{Sin } \left(\dfrac{\text{A} + \delta}{2} \right)}{\text{Sin } \dfrac{\text{A}}{2}}$. This equation requires the observations to be made and the data determined when the prism is in such a position that δ is a minimum (see fig. 3).

III. $\mu^2 = 1 + \left(\dfrac{\text{Cos A} + \text{Sin } i}{\text{Sin A}} \right)^2$ This equation re-

quires the prism to be in such a position that the angle r' (or, of course, r) is at its critical angle, while the data are being determined (see fig. 4).

$$\text{IV. } \mu^2 = \frac{\text{Sin}^2 i + \text{Sin}^2 i' + 2 \text{ Cos A Sin } i \text{ Sin } i'}{\text{Sin}^2 \text{A}}.$$

This equation holds good without any conditions being imposed upon the quantities involved whatever.

Part III.—The New Spectrometer.

The instrument is illustrated in fig. 5, and its main features may be tabulated as follows:—

a. Both tubes are auto-collimating telescopes, and both are made to rotate in the horizontal plane.

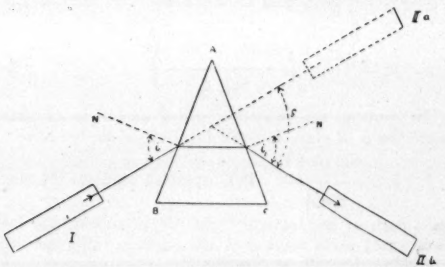

FIG. 3.

b. The illuminant is central above the prism table, and hence once set is constant on the slits whatever position they may occupy round the circle. There is a shade below to prevent stray light from interfering with the observations.

The advantages gained by these new features may, to start with, be briefly tabulated as follows:—

a. The refractive index of a prism can be determined whatever its angles may be, for the new instrument adapts itself for determining it by any of the following methods:—

(1) Position of normal incidence (fig. 2). (2) Mini-

mum deviation (fig. 3). (3) Critical incidence (fig. 4), or the (4) return path (Abbé) method.

(The equation for (1)—(7), Part II.—holds good for number (4) method also.)

b. By having two auto-collimating telescopes, not only is a check obtained by taking an observation first through one eye-piece and then through the other, but a further check is readily obtained by passing the light through the prism in a similar but opposite manner. Figs. 2 and 4 make this point clear.

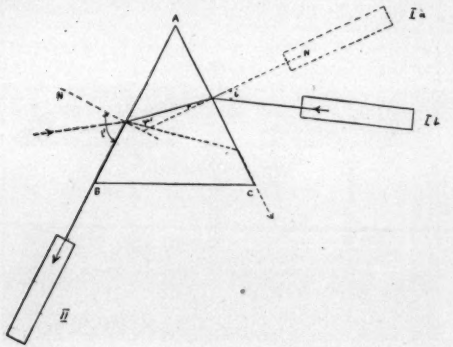

FIG. 4.

c. The error due to auto-collimation is eliminated when method (1) (normal incidence) is employed, by taking the mean of two observations—with illuminating prism and cross-wires to right and left, or *vice-versâ*, of field respectively. The same applies to the "critical incidence" method, which, however, is not apt to produce such accurate results owing to the difficulty met with in placing the cross-wires to exactly cut the half-light and half-dark field.

Fig. 13 illustrates the principle of eliminating this error, its amount, etc., is dealt with more fully later on.

In method (2) (minimum deviation) it is fairly obvious that the auto-collimating error cannot be elimin-

ated in this manner. However, it would not be a very difficult matter tc work out a table of correcting values for prisms of various refracting angles, and which would always be constant for the particular instrument. This, again, will be more clearly understood by what follows later on.

A section through the eye-piece and illuminating apparatus is shown in fig. 6, and it will be noticed that the light is reflected on to the slit by means of a mirror at approx. 45°. This has been found sufficient for pro-

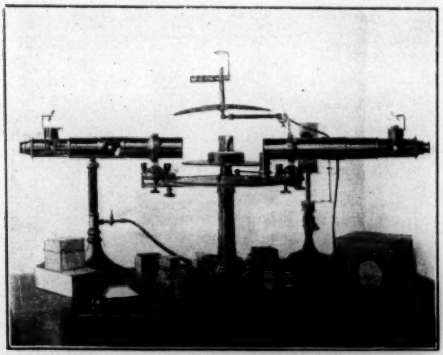

FIG. 5.

NEW DESIGN, "DUPLEX AUTO-COLLIMATING SPECTROMETER," BY VAL H. MACKINNEY.

ducing the bright spectra of sodium, lithium, and such like; but some special illuminating device must be employed when observations are required to be made of spectra of less intensity.

The theory of the new illuminating apparatus I propose to employ is illustrated in fig. 7, where A B represents the position the slit will occupy. In calculating the curves (which, by the way, are plane cylindrical, the axis of the lower one, which has the greater curvature. being perpendicular to that of the other) and size of this illuminating device, it is as well to note that the

angle of the cone of light falling on the slit should be the same as the angle subtended by the collimator objective at the slit—when same is focused for parallel light. Fig. 8 will make this point clear. The various methods indicated previously for determining the μ of glass, in some form of prism, are fully described in the many text-books on the subject, all that need be done here is to lay stress on the advantages offered by the new instrument, before dealing with the error introduced due to auto-collimation. They are :—

(1) The employment with ease of any of the four methods mentioned. (2) The accuracy of the observations. (3) The means it provides for eliminating the auto-collimating error directly when employing methods (1) or (3); and (4) The ease with which the observations may be doubly checked.

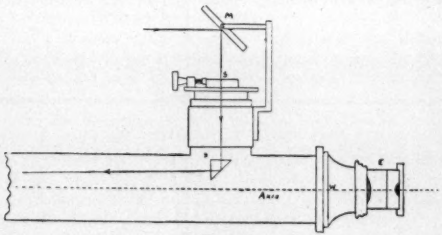

FIG. 6.

The Auto-Collimating Error.—One of two kinds, its amount and its correction.

When one auto-collimating telescope is employed and the light made to *return along its own path* by silvering the back surface of the prism (A, B, C, and D, fig. 12, show various sections through the Zeiss spectrometer auto-collimating telescope—designed specially for the employment of this method (4)) an error is introduced.

Suppose, as in the case of the Zeiss spectrometer, the illuminating prism occupies the upper half of the field and the cross-wires the lower, then from E, fig. 12 and fig. 13, it will be seen that for the return light to be

received on the cross-wires *the incident light cannot fall absolutely normally* (perpendicularly) *on to the face of the prism.*

First Kind of Error.—E., fig. 12, indicates the error introduced when the illuminating prism is in the upper half of the field—*the* δ *is measured for a prism of ap-*

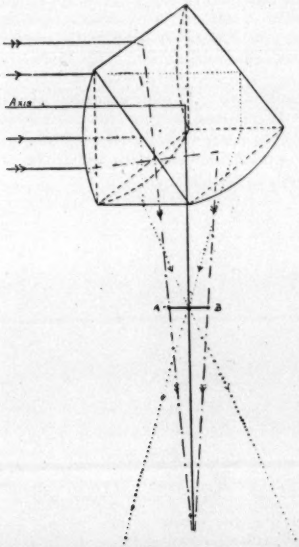

Fig. 7.

parently greater refracting angle (A) *than it really is* (this wooden model that I have here will clear this point) —and, therefore, than has been previously determined. Hence, the resultant μ, determined by means of the equation (7), Part II., will not be correct.

Second Kind of Error.—Fig. 13 indicates the error introduced when the illuminating prism occupies one side of the field—to right or left—and the cross-wires

the other. Now, our equation (7), Part II., requires the δ to be determined for either normal incidence or emergence, but absolute normal incidence or emergence cannot be detected, for the light in that case will not fall on the cross wires and enter the eye-piece. *Hence the δ measured is that for light of slightly greater or less incidence than normal incidence*, according to whether the prism is to the left or right of the field respectively.

From fig. 13 it will be seen that by taking observations through each eye-piece of the new spectrometer, one reading of δ may be obtained for incidence slightly in excess of the normal, and another may be obtained for incidence slightly under normal (nearer min. dev.), so that by taking the mean of these two readings we are able to eliminate the error otherwise introduced.

In the case of the min. dev. method of determining μ the error may be corrected, as before stated, by means of a table of correcting values. This I should propose to supply with each instrument.

The following determination—from practical data—of the effect of the auto-collimating error, on the final μ determined, will illustrate the necessity for its elimination.

Let the instrument be a single tube one, and the method employed the "return path" (Abbe) one, with the illuminating prism in the upper half of the field and the cross-wires below. Also let the working distance between these two be 10 mm., and their distance from the nodal point of the objective 300 mm.

Then from E, fig. 12, we have that tan, or, for all practical purposes,

$$\text{Sin } \alpha = \frac{5}{300} = .0166\dot{} = 57'$$

$$\therefore \text{Sin } \beta = \frac{57'}{\mu} \text{ (let } \mu = 1.5) = 38'$$

$$\text{Now A C} = \frac{\text{AB}}{\text{Sin C}} \text{ (let AB} = 10 \text{ mm.)}$$

$$= \frac{10}{.9999}$$

$$= 10{\cdot}0010001\dot{} \text{ mm.}$$

Hence, path through prism is lengthened (approx.) by 1/10,000th of its true length (A B), the length for which the equation employed is deduced. In other words, since β is small, the light apparently passes through a prism with a refracting angle 1/10,000th of its true angle (the angle measured) in excess.

FIG. 8.

The following readings—the mean of a number—were obtained by means of the Zeiss spectrometer:—

Angle of prism 29° 48′ 3″.

Normal incidence (practical) gave for sodium light δ, 24° 59′ 49″

$$\text{Therefore } \mu = \frac{\text{Sin } 54° 47' 52''}{\text{Sin } 29° 48' 3''}$$

$$= \frac{.81712}{.49698}$$

$$= 1.64417$$

The above δ, however, was really for a prism of refracting angle (A) of 29° 48′ 3″ + 1/10,000th of this = 29° 48′ 14″.

Therefore, the correct δ should be the above δ, 24° 59′ 49″ *less* 1/10,000th = 24° 59′ 40″, since an increase of A gives a corresponding increase in δ.

Therefore, with correction,

$$\mu = \frac{\text{Sin } 54° 47' 48''}{\text{Sin } 29° 48' 3''}$$

$$= \frac{.81705}{.49698}$$

$$= 1.64403$$

Note, the final μ determined, without allowing for the error, is wrong in the fourth decimal place, which is not permissible. The standard mentioned previously was that it should be *correct* to the fourth decimal place.

Part IV.—Appendices.

Appendix A.—Other quantities useful in specifying the refractive *properties of glass.*

The quantities already briefly mentioned are:—

(1) *Refractive Index*, for the mean of the sodium (D) lines, μ_D.

(2) *Mean Dispersion*, the difference between the indices for the F and C lines, $\triangle$.

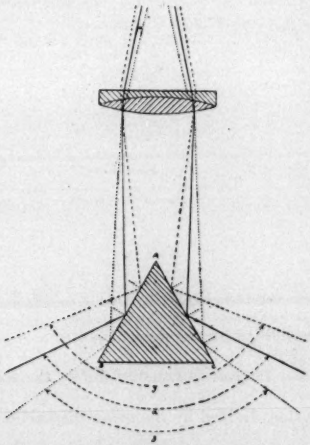

FIG. 9.

(3) *Dispersive Power*, the mean dispersion divided by the μ for the mean of the sodium lines, less the μ of the surrounding medium (air, whose μ for all practical purposes may be taken as unity.) $\omega = \dfrac{\triangle}{\mu_D - 1}$.

The following are also found necessary in practice for the purpose of more fully comparing the properties of the specimen under observation:—

(1) *Efficiency.*—The reciprocal of the dispersive power —sometimes known as the dispersive reciprocal, or dispersive weakness—,

$$\nu = \frac{\mu_D - 1}{\Delta}$$

(2) *First Partial Dispersion,* the difference between the indices for the A′ and D lines, δ_1.

(3) *Second Partial Dispersion,* the difference between the indices for the D and F lines, δ_2.

(4) *Third Partial Dispersion,* the difference between the indices for the F and G′ lines, δ_3.

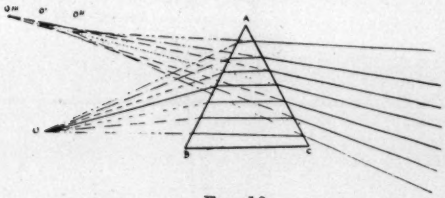

FIG. 10.

(5) *First Partial Dispersive Ratio,* the first partial dispersion divided by the mean dispersion, $\alpha = \frac{\delta_1}{\Delta}$.

(6) *Second Partial Dispersive Ratio,* the second partial dispersion divided by the mean dispersion, $\beta = \frac{\delta_2}{\Delta}$.

(7) *Third Partial Dispersive Ratio,* the third partial dispersion divided by the mean dispersion, $\gamma = \frac{\delta_3}{\Delta}$.

Appendix B.—Reasons for choosing parallel incident light — methods of testing same:—Error introduced, due to non-parallelism of the incident light, in determining μ.

Firstly, from a glance at fig. 9 it will be seen that unless the refracting angle of the prism under observation is determined for parallel incident light on to its two surfaces, the true refracting angle of the prism is not determined, and, therefore, an error is introduced into the final μ. The amount of this error is dealt with later.

Secondly, when parallel incident light is employed no "aberration of the rays"—as it is termed—takes

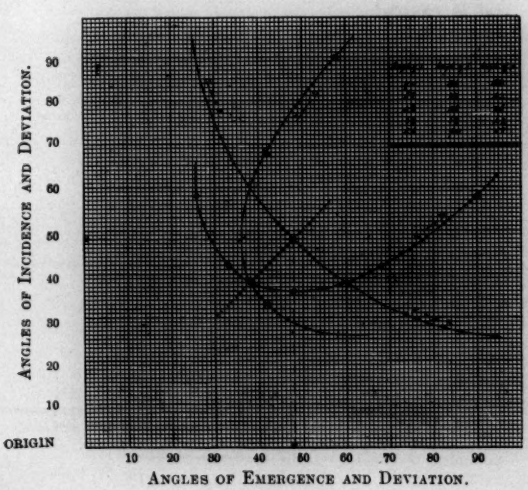

FIG. 11.

place, as is the case when convergent or divergent incident light is employed. This aberration is illustrated diagrammatically in fig. 10, and, naturally, produces want of sharpness in the final image of the slit. The aberration increases as the angle of the converging or diverging cone of light, and the greater the magnification of the telescope employed the greater will be the want of sharpness in the final image.

From the above it is obviously of considerable importance to be able to detect whether the light emerging from the collimator objective is perfectly parallel or not.

There are at our disposal the following two methods of testing the parallelism of the light emerging from the collimator. The first method is the one usually employed, but for delicacy the second cannot be surpassed and is worthy of more attention than it seems at present to receive.[1]

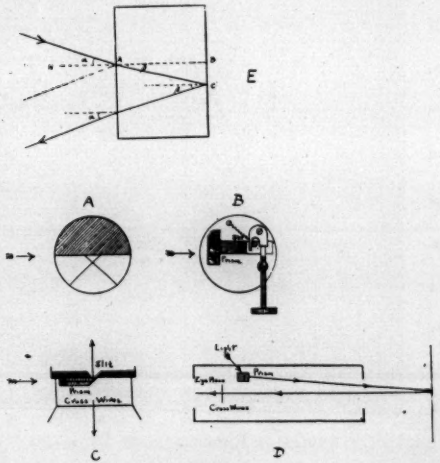

FIG. 12.

First Method.—Focus the telescope on some distant object, point it directly down the collimator, and then focus it on to the slit by moving the slit itself backwards and forwards. Whenever the telescope is focused

[1] I am indebted to Schuster and Lee's Exercises in Practical Physics for the principle of this (the second) method.

it should be accomplished by racking *in* so as to prevent, so far as possible, the accommodation of the eye being brought into play; and care should be taken to see that there is no parallax.

Second Method.—Place a prism in position on the spectrometer table and adjust the collimator and telescope alternately until they occupy such positions that —no matter at what angle the light is incident on the first surface of the prism—the image of the slit remains sharp. This method, perhaps, will be more clearly understood when it is stated that it is based on the fact that *for any but parallel light*, the image of the slit is formed at different distances from the prism, according to the obliquity of the incident light at the first surface of the prism.

The following practical example—from observations —will be found of interest as illustrating the extent to which the final μ may be affected by employing non-parallel incident light.

Parallel Light[2].—Length of collimator 245 mm. Angle of prism (to nearest half mm.) 52° 23′ 30″.

Divergent Light.—Length of collimator 241 mm. Angle of prism 52° 21′ 30″.

Convergent Light.—Length of collimator 249 mm. Angle of prism 52° 25′ 30″.

An alteration in the length of the particular collimator employed by 2 mm., produces, therefore (approximately), an alteration in the angle of the prism by 1 minute. In calculating the μ of a prism of approximately 30° refracting angle, firstly with and then without this 1 minute error, I obtained the two following values for μ:—

(1) $\mu = 1.561596$ (correct)

(2) $\mu = 1.561917$ (refracting angle decreased by 1 minute).

[2] Incident on first surface of prism (tested by Schuster and Lee's method with great care).

In calculating the μ of a prism of approximately 60° refracting angle in a similar manner I obtained—

(1) $\mu = 1.630890$ (correct)

(2) $\mu = 1.630518$ (refracting angle decreased by 1 minute).

The results show that the fourth decimal place is affected in the final μ by an error in the refracting angle of 1′, further, that the error increases as the total refracting angle decreases.

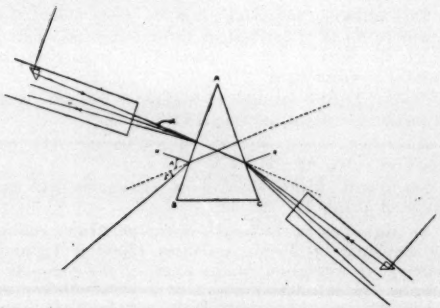

FIG. 13.

Appendix C.—To prove that the condition for minimum deviation is the same as that for symmetrical refraction through the prism.

There are three distinct ways in which the above can be proved. Firstly, it can be proved by logical reasoning; secondly, mathematically; and thirdly, graphically.

Taking the proofs in the order mentioned we have:—

1.—Suppose for the moment that the minimum deviation does not occur when the angle of incidence is such as to cause a symmetrical course through the prism, in other words, give the angle i (fig. 1) some other value.

In this case the angle of emergence, i', is no longer

equal to the angle of incidence, i, but light, we know, is capable of being made to retrace its path—in which case i' will become i, and i, i'. No possible alteration can, of course, take place in the deviation. Hence, it is possible to obtain a minimum deviation with at least two *different* angles of incident, but this we know from observation to be impossible. Therefore, we come to the conclusion that the mimimum deviation of the path of the wave of light must take place when the light passes symmetrically through the prism.

2.—The complete mathematical proof is given in Heath's excellent book on Geometrical Optics, and need not be repeated here.

3.—The graphical proof, I believe, is somewhat new—at least, personally, I have not seen or heard of it before. It is ably illustrated in the curves shown in fig. 11—which I have plotted from calculations and checked by observations. Their shapes will alter slightly, of course, with different prisms, but they will always retain their symmetry one with another. In other respects I think they are self-explanatory.

Appendix D.—Conclusion.

One further word can be said in favour of the new instrument. When it is employed for mapping spectra the necessity for an extra tube (for projecting the image of the divided scale simultaneously into the field with the spectrum under observation) is eliminated by fixing the divided scale in the place of one of the slits, and a small mirror on the edge of the prism table for reflecting the light back on to the cross-wires. The mirror must be capable of rotation, and of being adjusted to tilt in either direction.

As a final conclusion I cannot too heartily thank Messrs. R. and J. Beck for the really noble way in which they have put together my ideas entirely at their own expense, and I trust that your criticism of my endeavours will be searching and thorough—as well for their sakes as for my own.

THE OPTICAL CLASSES AT THE NORTHAMPTON INSTITUTE.

REPORT BY DR. R. MULLINEUX WALMSLEY, F.R.S.E.

The foundation of the classes in technical optics at the Northampton Institute synchronised, by a pure chance, with the starting of the certification scheme of the Spectacle Makers Company. The first real session of the Institute's work was in the winter of 1897-8, and this was devoted to the development of the engineering classes, a preliminary scheme for optical education being suppressed for the time being owing to the pressure of the other work. The scheme was taken up again in the early summer of 1898 to be put into form for the following winter, but before much progress had been made the proposed establishment of examinations by the S.M.C. came to my knowledge. Negotiations were opened with the Company and a conference was held in July, 1898, on the subject.

Regarding the syllabuses of the S.M.C. as an indication of the immediate requirements of the trade, the first classes to be established were such as were required for the certification scheme. These were classes in Visual Optics, in Applied Optics, and in Optical and Scientific Instruments; a special feature was made of laboratory work as in the electrical engineering classes established in the preceding winter. The classes attracted nearly one hundred students, a number which was not maintained in the years immediately following, but has since been exceeded. The falling off was, of course, due to the natural diminution after the first rush of Opticians to take advantage of the certification scheme.

(A) Work of the Classes and its Development.

This Opticians' course was in the following session amplified by the addition of a workshop class in *lens grinding*, and one in *spectacle making, glazing, and repairing*, the number attending the workshop in that

year being 27. The Opticians' course has continued to be the principal course for evening students, but it was found necessary in 1904-5 to divide the course into two —Preliminary and Applied Optics—sections; the former is intended for the younger members of the trade, and it is hoped that it will be taken by apprentices and assistants immediately on entering the trade; the latter section is a more advanced course to be taken by those who have had considerable experience in the optical industry, and have a good practical knowledge of ordinary optical work.

The Visual Optics class, which has from the beginning formed part of the Opticians' course, is now taken with the second section, and has since 1899-1900 been under the charge of a qualified medical practitioner. It has covered the syllabus of the S.M.C. examination from its inception.

In 1904-5 a special laboratory course in practical sight testing was inaugurated; this class was continued in the following session, but owing to the impossibility of guaranteeing sufficient practical work in subjective testing the scope of the class has been modified, and special attention is now paid to the objective methods of measuring the refraction of the eye and its other optical properties. To deal with the instrumental side a short course of lectures on the instruments used has been added this winter. At the same time a special course in the physiology of the eye was instituted (Physiological Optics B) under Dr. Ettles; the aim of this course is to give a more thorough knowledge of the physiological and optical properties of the eye; it employs the ordinary methods used in teaching general physiology.

The Scientific Instruments class has undergone some modification. Originally intended to give those interested in the sale of instruments some real knowledge of the instruments they sold, it has developed so that it now appeals also to those engaged in instrument-making or employed by instrument makers. For some years it covered the requirements of the S.M.C. examination as regards a special instrument, but since the modification of the syllabus the requirements of this examination have been much below the standard of this course, and it has been taken only by those students who wish to have a special knowledge of optical instruments, or are en-

gaged in their manufacture. The returns show that for a number of years it was customary for students to take the one section of this class which they were offering at the S.M.C. examination. The requirements of these students are now dealt with in the second year's, or advanced class, of Applied Optics.

In the interests of the instrument-making side of the industry a class in the Design of Optical Instruments was begun in 1902-3; it was divided in 1904-5 into a general class and a class for the design of telescopes. In each of these sections special laboratory and workshop work has been arranged.

In 1899-1900 a course on Colour with special reference to Tri-Colour photography was instituted. This course was continued in conjunction with the L.C.C. School of Photography till 1904-5, when, owing to re-arrangements, it was transferred to Bolt Court. It is proposed, however, to establish a course dealing with other technical applications of colour measurements as soon as practicable.

(B) Relation of Classes to Examinations.

(1) S.M.C. Examination.—*Visual Optics.*—The lecture course covers the syllabus of the examination while the laboratory work is much wider than is required. It is impossible to say how far the laboratory work could be tested in the actual examination, except that the testimony of the examiners is, that the men so trained have a more thorough knowledge of optical work than other candidates. The class in *Applied Optics* covers rather more than the syllabus of the examination, the reason being that this class is the basis of all the optical work of the department.

Scientific Instruments.—As this class is intended for those whose business it is to make, test, or sell optical instruments as well as the retail Optician, the subjects are treated more thoroughly than is possible in the examination.

Physiological Optics A.—In this case the aim is not so much to give proficiency in sight testing as to show the various methods used in objective testing and to explain the various instruments both in construction and use. It deals to a considerable extent with normal eyes and their appearance.

Physiological Optics B.—This class does not claim to deal with the subject matter of any examination.

General.—The change in the syllabus by which the special subject was excluded for a vaguer general knowledge of instruments has made it more difficult for our classes to meet the requirements of the examination. It has deprived our instrument class of a number of possible students.

(2) B.O.A. EXAMINATION.—*D.B.O.A.*—Theoretical section and practical section (lenses) are covered by the courses in Applied and Visual Optics. Again, the Institute courses are more thorough than is required for the examination. The Subjective testing section is covered by the class in Physiological Optics A.

F.B.O.A.—The subject matter actually required for this examination is practically included in the classes Physiological Optics A. and B., but these courses are much wider than the practice of the examination. Judged by the syllabus it would seem that the F.B.O.A. requires a student to go beyond the subject matter of the classes in Physiological Optics A. and B., but the questions set do not bear out this view.

General.—The knowledge actually required at the examination does not seem to be so thorough as that given in the Institute courses.

(C) Organisation.

In the sessions 1898-9 to 1902-3 the work of the optical classes was carried on under the control of the Applied Physics Department, and up to the present time the drawing classes and the instrument making workshop, as far as relates to the metal work, are carried on by the Mechanical and Electrical Engineering Departments of the Institute.

At the beginning of the session 1903-4 a separate department of Technical Optics was created, with a responsible Head devoting his whole time to the work, and in the following session Day Classes were inaugurated. The staff of the department at present consists of the above: Head, a Chief Assistant, a Demonstrator, a Lecture Assistant, and a Laboratory Assistant. In addition the evening classes have the services of three Lecturers and a Workshop Instructor.

(D) Trade Support.

The S.M.C. contributed to the support of the optical classes the fee of the lecturer in Visual Optics for the sessions 1898-9 and 1899-1900, and the fee of the workshop instructor in the latter session; it also assisted by a loan of apparatus which is still available for the work of the Institute.

In 1900 the trade, through the Optical Society, subscribed the sum of 50 guineas to assist the classes, and continued the contribution in 1901. This sum was doubled in 1902, continued at this amount in 1903 and 1904, and last year, 1905, reached one hundred and ten guineas.

The Optical Society's Education Committee met in 1901-2 and reported in June, 1902. The work of this Committee was of service to the Institute in indicating the actual requirements of the trade, though with the present acommodation it is not possible to carry out all the suggestions received.

The foundation of the Aitchison Scholarship in 1904-5 is also evidence of the interest the work of the Institute has excited, but the response in the number of candidates competing shows the lukewarmness with which real optical education is regarded in this country.

R. MULLINEUX WALMSLEY,

Principal.

February 16, 1906.

PRESIDENTIAL ADDRESS.

By R. Mullineux Walmsley, D.Sc., F.R.S.Ed.

Although, at the recent Annual General Meeting, I briefly expressed my appreciation of the confidence that the Society has shown in me and the honour that they have done me in electing me for a second time to preside over their meetings and over the welfare of the Society, I wish here and now more formally to put on record that I do indeed very much appreciate my re-election. I can only assure my fellow members that, in accepting this mark of their continued confidence, I shall endeavour during my ensuing year of office to do all I can to promote the welfare of the Society.

In my first occupancy of this chair I succeeded the able and energetic first President, Mr. W. H. E. Thornthwaite, whose pilotage of the infant Society, through many initial difficulties, we all gratefully remember. Now, my not less ardouous task is to follow in the Presidential chair so well known and able a President as Professor Silvanus P. Thompson. I am quite sure I shall carry the opinions of all present with me when I say that, in a way which we all feel must conduce and redound to the efficiency of the Society's work, his scientific eminence and position have helped to keep the Society well before the scientific world as a serious scientific Society with perfectly definite aims and intentions. To the varied services to the Society during his year of office which were referred to by the mover and seconder of the official vote of thanks at the last meeting, may I add the expression of my own opinion that as the official representative of the Society at the Optical Convention no happier choice could have been made. At the present moment, however, my mind reverts more strongly to his Presidential Address, which all here must recollect took the form of a very fully illustrated and delightful lecture upon the early literature of Optics, the

interest of which I cannot hope to equal in my present effort.

Before, however, proceeding with the main subjects of my address I crave your indulgence whilst I pause for a moment to refer to the great loss which the whole world of optics sustained in 1905 in the death of Professor Ernst Abbe. Born the son of a mill foreman he rose by sheer force of hard work, intellect and a commanding personality, to a position in which, with a world-wide reputation, he was recognised as a practical and scientific leader in the optical industry, who not only had done remarkable work himself, but still more had influenced and stimulated others to a fruitful productiveness in the same direction. Starting entirely on the academical side as a student and as a teacher of mathematics, physics, and astronomy in German universities, his first efforts in the application of his training to the requirements of applied science were due to the necessities of an instrument maker whose name has since become as well known as that of Abbe himself, I refer to Carl Zeiss, the celebrated optical instrument maker of Jena.

This partnership, started in 1866, of the practical man and the man of science, produced remarkable results, which must be familiar to most of my hearers, and when a few years later the firm was joined by a glass-maker, Dr. Otto Schott, first of Witten and afterwards of Jena, the combination of science with industry was complete, and it only needed the fostering care and liberal help of a wise government to produce, by direct experiment on a commercial scale and working towards definite ends, the remarkable results with which the whole scientific world is familiar. The history of this tripartite combination and the fostering of its efforts by the application of public funds is an unique object lesson as to what can be done by the proper amalgamation of these various resources, and I would like to commend, not only to the members of the industry, but to those who control the application of public money to educational and trade purposes, the very serious study of this example. It would take me too long, nor do I suppose it necessary here, for me to go through the varied aspects of Abbe's work and that of the band of workers associated with him.

I have named the original triumvirate, but it is well known that other scientific workers, very efficient for their various purposes, joined the original band, and it is the work of the whole that has made Jena famous throughout the world as an optical centre. The method, however, by which such a combination of workers was drawn together is worthy of very careful observation. Put shortly, the pivot upon which the whole progress and development turned was the existence of a well-planned optical research laboratory to which any optical worker who could prove his ability to do good optical work was welcomed with practically open arms. In this laboratory no expense was spared. Everything that could reasonably be desired was placed at the disposal of the earnest worker, and those who provided the means were content to wait for the return which they were certain would come. In the results referred to above what a commentary we have on the wisdom of such a course! I am glad to say that I have seen indications that the utility of similar methods is beginning to be recognised in England, and I know at least one instance of their successful application, though on a much smaller scale. Still less have I time to devote to Abbe's work in the direction of associating the actual workers in the industry with the scientific leaders and the administrative directors, but his work in this direction will repay careful study. In other words, the development of the Carl Zeiss Stiftung forms no less remarkable a study on the general industrial side than does the other aspect of the work alluded to above in the direction of the combination of science and industry.

In taking the chair for the second time one's thoughts naturally revert to the ideals which one had in one's mind on the first occasion of being honoured with the Society's confidence, and to refresh my memory in this respect I have referred to my previous address, and would crave your indulgence whilst I direct your attention to one or two points. As already remarked, the Society was then in but the second year of its existence, and it was, therefore, not unnatural for a desire to be expressed that in its development it should include all those interested in optical matters, and especially that, in its trade members, it should throw its mantle sufficiently broadcast to cover all sections of the trade. Of course,

it was well known that some sections were specially taken charge of by existing and older societies, but it was hoped that the Optical Society could be made a common meeting ground for all who were interested in the study of optics either from its technical or from its theoretical side.

Now, although our numbers are still small I venture to think that this early aspiration has had a fair amount of fulfilment and that we do at the present time embrace within our numbers representative members of all sections of the trade on the one hand, and, further, that we have, especially when we look at the list of honorary members, some of the foremost scientific exponents of optics gathered within our fold. This, I submit, is no light achievement to have accomplished in so short a period. Still more, and as a consequence of this, we have done much to bring the various sections together and have done something to promote a wider tolerance between those holding diverse views—a tolerance which, it may be hoped, will continue to spread and fructify in effective combination and work for the common good.

Another desire expressed in my previous address was that it might be found possible, within a short period, to hold an exhibition of optical goods of British origin which should convince the somewhat sceptical English industrial and other worlds that the optical appliances offered for their acceptance were not, by a long way, all made in foreign countries. (Cheers.) The year in which this aspiration was uttered was the year of the great Paris exhibition, and we had at that exhibition, in the German exhibit, an example of what could be done by united effort on the part of a trade and of the scientists interested in the applications of physical science. This exhibit, with its splendid catalogue, formed an object lesson which could well be taken to heart. As you all know the suggestion eventually bore fruit, but the plant was of slow growth, and it was not until last year, that is five years after the suggestion had been made, that it culminated, thanks to the labours of many enthusiastic workers, in the carrying out of a scheme, which, in its aims and magnitude, more than realised the hopes which had been expressed.

Although the Optical Convention of 1905 was very

fully referred to in the remarks of the Chairman at our Annual General Meeting in presenting the annual report of the Council to the Society, I cannot refrain from again directing your attention to the subject. Whether we look at it from the scientific, from the trade, or from the social aspect, I think every one who came into contact with the Convention during its proceedings must admit that it was an unqualified success. (Cheers.) I am perfectly aware that in order to widen the work of the Convention its father, the Optical Society, stood on one side and that it was in more than form an independent organisation. But when we consider that it was presided over by a Past President of the Society, Dr. Glazebrook, the honoured Director of the National Physical Laboratory, and that the initiatory steps were taken during his year of office as President, and in the Council Room of the Society, I think it is not too much to say that had it not been for the existence of this Society the Convention would never have been held. I would remind you, partly in order that it may appear upon our records, that this Convention consisted of an unparalleled display of British optical goods, a display which opened the eyes of all who saw it to the importance of the optical industry as a branch of British industrial effort.

On the other side, for four days the most eminent physicists of the day, in combination with leading members of the industry, presented papers for discussion upon a great variety of matters directly affecting the future development as well as the present progress of that industry. So much did the supply of papers exceed our expectations that on three out of the four days the Convention had to divide itself into two sections to get through the work which was placed before it. One reflex action of this commendable activity in the production of optical papers was deplored by our Honorary Secretary at the last meeting. I refer to the temporary drying up of the supply of papers for this Society's meetings during the present winter. Though we, of course, sympathise with our Secretary in the difficulties which he has, therefore, had to face, I think that the members of the Society will agree with me that we are more than willing to put up with the temporary inconvenience in view of the substantial results obtained at the Convention itself. The permanent record is em-

bodied in the "Transactions" which will shortly appear, and in the Optical Convention Catalogue, in which an attempt was made, and not, I venture to think, unsuccessfully made, to produce a work equal to that which marked the German exhibit referred to above. These remain to testify to the character and accomplished work of the first British Optical Convention, and may be an incentive to our immediate successors to follow in our steps at no very distant date. On the social side, too, I venture to think that this Convention has left a permanent trace upon the industry in bringing together, under the most favourable conditions, and more effectively than could be done by the efforts of a single society, different interests, which, in the ordinary course, seldom come into contact with one another. Had this Convention alone been the outcome of the formation of this Society some seven years ago, it would have amply justified all the work and labour which the self-sacrificing founders of the Society devoted to its early development.

Another matter, and one not less important in my opinion to the future welfare of the industry, was put forward in my previous address, and that was the formation of an Optical Standards Committee. This Committee, as you are all aware, was formed during my year of office, and includes amongst its members a very strong body of scientific and practical men. For working purposes it is divided into various Sub-Committees who have power to add to their numbers men specially interested in the work to be undertaken, but who are not upon the Standards Committee itself. It may be of interest to note here the titles of these different Sub-Committees. They are as follows:—Spectacles and Eye-Glasses, Opera and Field Glasses and Telescopes, Nautical and Surveying Instruments, Optical Glass, Photographic Apparatus, Trial Cases. In the first instance other Sub-Committees were formed, but after careful enquiries they found that the ground that they proposed to take up was being well covered by other organisations, and, therefore, as it was never the intention of the Optical Society that its work should overlap work which was being done well elsewhere, these committees were quietly disbanded.

All these Sub-Committees named have done work, some more, some less. The only one which has produced a complete report is the Spectacles and Eye-Glasses Sub-Com-

mittee, and its report, after being adopted by the principal Committee, has been discussed and adopted by the Society and recommended for the acceptance of the optical world. As members of the Society are aware, the results of the labours of this Sub-Committee have not yet been generally adopted, but special efforts are being made to accomplish this desirable object, and I venture to hope that during my year of office some definite advance may be made in this direction. (Hear, hear.) It is a commonplace to say that it is always difficult to persuade an established industry, which has grown up gradually in disconnected sections, to alter its heterogeneous standards and to adopt others which have been carefully thought out in the light of the most advanced experience and science available, however authoritatively and strongly these new standards may be put forward. I may claim, without exaggeration, that the standards recommended in this subject by the Committee and the Society have been arrived at only after exhaustive discussions, in which all sides of every question have been thoroughly examined and the attempt honestly made to draft the standards so as to minimise whatever inconvenience may arise from their adoption in particular instances. It is, therefore, to be hoped that some *modus vivendi* may be found and that ere long these standards will be as generally accepted by the trade as the corresponding electrical standards are now accepted by the electrical industry. It is not too much to say that these common electrical standards which are used in all countries and in branches of the industry have had more effect on the enormous development of that industry during the last twenty or twenty-five years than any other single factor that can possibly be named. I would, therefore, appeal with all the force of which I am capable to the practical instincts of the members of this Society and to the trade generally, to see to it that some means of conferring similar benefits upon the optical industry should very speedily be found.

Turning now to the other Sub-Committees, it may be asked, "why have they not also produced reports which the Standards Committee could submit to the Optical Society?" The reasons are various. The chief cause of delay is, that as from the nature of the case, these Committees are formed of men in the forefront of the work

whose time is very fully occupied indeed, it is extremely difficulty to get attendance at meetings and the carrying out of the other work without which no substantial report can be presented. The choice really lies between forming Sub-Committees of such men and facing the disadvantages which inevitably follow, and forming them of less busy men, who, whilst they might be competent to some extent to advise, would not be of sufficient standing in the various subjects on which they were called upon to advise, for their reports to carry much weight with the industry. It is, in fact, the choice between strong reports well backed but delayed, perhaps, even for an unconscionable time, and feeble reports which would not have any chance of producing a practical result.

This, as I have said, is the first reason for delay, but there are others. In some cases the need for standardisation is not very urgent. Then, again, and this especially applies to the Sub-Committee on Optical Glass, no recommendations can be made until elaborate experiments have been concluded. Such experiments are being conducted under the auspices of this particular Sub-Committee and the Society has generously granted the necessary funds. The work, however, can only proceed slowly. Then, again, during the past eighteen months the energies of the members of the Committee and its Sub-Committees have been devoted to the work of the Optical Convention, and other work has had to be suspended for the reason given above, namely, that the time which can be obtained from the members is a strictly limited quantity.

To critics who may be inclined to be severe on the subject of these delays I would venture to remark that similar delays are of every day occurrence wherever subjects of this kind have to be handled by Committees of busy public men. To give an instance which has occurred quite recently. There is in existence a Steam Engine Research Committee formed by the Institution of Mechanical Engineers, and that Committee has recently brought before the Institution the results of eight years' experiments in connection with one part only of its work, and upon these results valuable discussions have taken place, but the experiments are even yet not complete, and the work of the Committee is far from being accomplished.

It is rash to make promises, but I do hope that the year's work upon which the Society is now entering may be marked by a further development of the work of the Optical Standards Committee, and one or two preliminary suggestions will be made by your Council this evening. One piece of work, indeed, is ripe for immediate action. At the meeting of the Society held on January 18 we were told by Mr. Selby that the National Physical Laboratory has now practically completed its arrangements for the testing of trial cases, and that only one or two points which he mentioned remain to be settled to place the Laboratory in a position to commence this important work. The "Trial Cases Sub-Committee" will, doubtless, take an early opportunity of conferring with Mr. Selby and the authorities of the Laboratory, and we hope that no long time will elapse before trial cases, with the N.P.L. certificates attached, will be obtainable by practising Opticians, a result which it may be expected will redound to the advantage of all concerned. (Hear, hear.)

Another direction in which one's thoughts inevitably trend on an occasion like the present is to examine not only how the ideals put forward on a previous occasion have been carried to realisation, but also to review the events which have occurred in the intervening period so far as those events affect the particular society. I do not propose to detain you long, but there are two or three outstanding events which, I think, are deserving of some prominence. The first in order of time, and probably also of importance, is the work of the Educational Committee of this Society, which was formed in 1901, and during the winter of 1901-2 held many deliberations, eventually producing a valuable report. The effect of this report is still being felt. Its immediate outcome, so far as the outer world could perceive, was the formation of a deputation to the late Technical Education Board of the London County Council to urge the importance of developing optical education in the Metropolis. Owing to the wholesale changes in the public administration of educational funds which synchronised with the visit of this deputation at the close of 1902, matters have been delayed through no fault either of the education authorities or of the Optical Society. It, therefore, happened that on account of these delays

the Optical Convention was in time to reinforce the appeal of the earlier deputation by organising a second deputation, which waited quite recently upon the successors of the Technical Education Board to emphasise the appeal of its predecessor and to urge again upon the authorities the necessity for immediate action. That deputation, as you are aware, was very favourably received, and I believe I am not going too far in saying that there is at the present moment a good prospect that its efforts will bear fruit at no very distant period. In fact, I venture to hope that during my period of office the scheme for the establishment, under some form or other, of an Institute of Technical Optics, will take tangible shape. (Hear, hear.)

The second event to which I should like to refer very briefly is of immediate domestic interest to the Society. It is the transfer of its headquarters from Bolt Court to Hanover Square. I do not know that the time has yet come when we can very well weigh the effect of this transfer, but my own personal opinion is that it has been advantageous to the Society—(hear, hear)—and that the increased expense of the accommodation here is an expense which has been well justified.

Lastly, in this connection, I would refer to the important development of the Society's organisation made during the Presidentship of Dr. Drysdale in the formation of a Students' Section. It has been the fashion to deplore the small membership of the Society, but I would ask those who criticise us in this way to remember that in forming a Society of this kind, and in criticising its development, the limitations imposed by human nature and historical setting must not be overlooked. Appealing to a trade to which the idea of combination and mutual help was altogether new, or, at any rate, new to the great majority, it is not surprising to find that those older members of the trade, who, throughout their business lives had got on, as they thought, very well without such combinations, should look askance at the new development. To such it was a practical impossibility, in many cases, that they should realise the great changes of the last twenty or thirty years and the necessity for new methods of handling problems which were beyond the power of any single individual to grapple with successfully.

I believe that it is this innate conservatism which has done more than anything else to keep down the numbers of the Society, and, therefore, I think that the formation of a students' section is of the very greatest importance to the future welfare of the Society. It appeals to those who, entering the trade, are not affected by the considerations to which I have just referred. They, at any rate, will, from the start of their professional or business career, be familiarised with the idea of co-operation with their fellows, with the further idea that the too close keeping of trade secrets is not advantageous, even to the man who keeps the secret, and still less to the welfare of the trade as a whole. With this knowledge unconsciously acquired and imbibed, it is to be hoped that as these student members arrive at years of maturity the membership of the Society will continue to increase and its influence will permeate more and more throughout the rank and file of the industry.

Turning now to other topics it may be of interest to the Society in the present position of optical education in London and in this country, if I devote some part of my address to considering in detail various matters connected with the technical education of Opticians, as seen entirely from the technical and educational and not from the administrative standpoint. In what follows I shall draw largely upon and illustrate my remarks by reference to the work done at the Northampton Institute, not because I wish to magnify or bring into undue prominence the work of an institution for which I am personally responsible, but because I venture to think that the only extended experience of education in this particular direction up to the present has been acquired there.

Educational work in technical optics, as most of my hearers know, divides itself into lecture, laboratory, workshop and drawing office work. In regard to the first of these, namely, the lecture work, I have little or nothing to add to the particulars with which I think most members of the Society will be familiar, as they have appeared for years in the "Announcements" of the Institute. The lectures at present cover the requirements of the various students who have so far offered themselves for instruction in technical optics, but it is not to be assumed that the whole ground has yet been

taken possession of in these syllabuses. Originally drawn up to cover the requirements of the certification scheme of the Spectacle Makers Company, those requirements being interpreted very liberally, the syllabuses have been extended from time to time and are still in process of growth, a process which is likely to continue for some time to come, if not always, should they continue to be successful.

But the work at the Northampton Institute to which we attach by far the greatest importance, is not that which is done in the lecture room, valuable though we believe that to be, but the work in the other directions referred to above, and especially in the laboratory. With our large experience of technical education in applied physics in other directions, we are convinced that nothing brings home to the mind of the student the realities of the subject which he is studying so much as good, sound, well-designed and well-thought-out laboratory work. So much did we believe in this in the early days that, as events have afterwards proved, we seriously crippled ourselves in lecture room accommodation in order to devote as much of the available space as possible to the physical laboratories. We do not regret this, for without a good laboratory course the knowledge of the student, obtained from books or even from lectures, remains, to a great extent, something outside himself. Without this he has a kind of faith-acquaintance and belief in the various laws which are brought before his notice, but he has no real vital apprehension of those laws, or of their consequences.

More especially he has no real idea of the metric side of the science, by which I mean that his magnitudes may be enormously wrong in any given problem without him realising that they are so. This is especially the case wherever the things measured are not quantities with which he is in the habit of dealing in his ordinary daily life. For instance, it is quite easy in an electrical problem involving such quantities, and also in some optical problems, to be many thousands of times wrong without the error being obvious to a merely theoretical student.

Such mistakes can only escape immediate detection by their authors when those authors are students who have never measured the quantities for themselves. No one in ordinary life would think of giving in as a correct

answer to a problem that the weight of a certain small piece of material is so many tons, when as a matter of fact he knows from the conditions of the problem that it can only be a few ounces. Corresponding mistakes, however, can be made in physical science without the authors of the mistakes being at all aware of the absurdity of the results which they are putting forward.

Again, the terms and expressions which are used in the science can only become living things to the students by actual experiment, and, wherever possible, by actual measurements of the quantities to which they refer. Even, to take a familiar optical case, a length of 50 centimetres or a curvature of 2 diopters may be glibly talked about by a student who has obtained all his knowledge from books or lectures, but it is only when he makes measurements in centimetres and draws curves with a curvature of 2 diopters that he begins to realise and be familiar with what the actual quantities mean. The most elementary work in the physical laboratory becomes thus of enormous educative value to the young student in any branch of physics, and not less in optics than in any other.

When the student has passed the elementary stage, and is capable of handling more elaborate apparatus, the experience he obtains in accurate measurement is not only in itself valuable for trade purposes but is still more valuable as a mental training in accurate thought.

Not only, however, should he be practised in the optical laboratory in measurements involving the highest accuracy, but he should also learn to break down, as it were, those elaborate methods of measurement to simpler ones, which, while sacrificing accuracy in certain directions still are sufficiently accurate for many of the purposes of the factory and of practical life. In fact, there is always a danger, especially with junior students, of carrying a measurement, and especially a calculation based upon it, to a degree of accuracy not justified by the fundamental data available or the requirements of the problem. The power of recognising when a measurement has been made with an accuracy which is all that the circumstances of the moment require or the conditions of the problem justify is, I need scarcely say, in this assembly of practical men, of enormous importance to the student. We have, therefore, developed in our

laboratories not only methods by which high accuracy can be attained, but also those which are better adapted to the rough and tumble life of the commercial workshop in many of its operations.

Another purpose for which the laboratories can be used by the trade, and are being so used, is to familiarise the seller, and even the maker, of optical instruments of the more complicated type with the actual use of those instruments. If makers and sellers could themselves use their own instruments more frequently and apply them to the actual objects for which they are designed, not under the conditions of the laboratory or of workshop test room, but under the conditions of actual practice, I am convinced that the development of many instruments towards perfection would proceed a good deal more rapidly. When, as often happens, the user of the instrument, who feels its defects, knows nothing of how those defects can be overcome, and when, on the other hand, the maker of the instrument is not the one who feels the shoe pinching in the use of it, it is obvious that the improvement of the instrument cannot be expected to advance at a very rapid rate. We, therefore, hold that in a good laboratory course in optics both makers and sellers of instruments should be practised in their use as nearly as possible under the ordinary conditions in which the purchasers of the instruments have to use them.

It must not, of course, be supposed that I am claiming that at the present moment the optical laboratories of the Northampton Institute are completely equipped to carry into effect the above ideals in all directions, or that we are satisfied with the equipment in all its details. Much still remains to be done which can only be accomplished when we remove into new quarters where additional space will be at our disposal. Still, I think I may claim that a very good start has been made, and that we can now offer laboratory training in technical optics which certainly cannot be surpassed in any educational establishment with which I am acquainted.

At the risk of wearying my hearers, but perhaps as having some permanent interest in itself, I have ventured to tabulate in an appendix the experiments at present available, or very shortly to be available, in the optical laboratories of the Institute. These experiments

are classified under headings and sub-headings according to the various courses for which they are adapted. Commencing with the simplest experiments for students who are new to the study of optics and to laboratory work, they proceed by easy stages through the less elementary work to more and more difficult experiments and testing, until the most advanced and standardising tests are reached. The main courses are:—1. Preliminary Optics. 2. Applied Optics. 3. Examination of Optical Instruments. 4. Use of Optical Instruments. 5. Physiological Optics. 6. Advanced Physiological Optics. 7. Design of Optical Instruments.

At a certain stage of the work the student leaves the experiments which should be done in common by all optical students, and begins to specialise in the particular work in which he is most interested. Indeed, as a rule, some of this specialising work is taken up somewhat earlier and carried on concurrently with the other, for the students cannot be put into water-tight compartments. For instance, those who are most interested in spectacles and eye-glasses will carry on the more elementary stages of physiological optics whilst still engaged upon their applied optics work, with which also the work classified as the "Examination of Optical Instruments" can be taken. For the rest a careful study of the details of the various courses will reveal the plan of the work, and will, I hope, justify the claims I have put forward regarding its importance and value. When further space becomes available it is proposed to add to these courses, courses on chemistry as applied to optics and optical materials, and to devote special laboratories to the chief branches of the industry, which are at present included as sub-sections in some of the above divisions. Such special laboratories will contain the testing outfit of a well designed testing-room in the corresponding optical factory, together with ample provision for research. In the present equipment these objects are being kept steadily in view and much of the equipment already obtained is adaptable to these purposes.

I have prepared a list of the various experiments which have been carried out at the Northampton Institute, but I do not propose to weary you by going through them. This was one of my reasons for getting the Appendix to my address printed beforehand, so that you might

absorb the general plan of the work. Dealing with the question of extra space, it may interest the members of the Society to know that the Optical Classes at the Northampton Institute will, next winter, be taken into another building, the British Horological Institute, nearly the whole of which will be devoted to classes in Optics.

Though, so far, reference has only been made to the use of the laboratory for instruction, a glance through the list of experiments will reveal that much of the apparatus in the advanced courses is well adapted for research work, and no assurance of mine is needed that under the direction of Mr. Chalmers the opportunities so provided will not be neglected, but that capable students will be encouraged to embark on original investigations which are no less valuable from the educational than from the industrial and scientific points of view.

From the laboratory work we pass naturally to the workshop instruction, which is also regarded as of great importance to optical students of all grades. It is obviously of importance to the worker at the bench, who can, in an educational workshop, be introduced to, and become to some extent familiar with, a wider range of work than is possible in a commercial workshop where his labour has to be utilised to the best monetary advantage by his employer. It must not be supposed, however, that the school workshop, unless the work partakes of the nature of a trade school, and even then only in a modified degree, can turn out finished and expert workmen. Certainly it is not possible to do so in evening classes. When one considers that the evening class session extends only over some 30 to 35 weeks of the year, it is obvious that an attendance of a couple of hours' per week can amount to little more than one complete week's work in a commercial workshop. The aim, therefore, of such a workshop for artisan students attending evening classes can only be that which has been stated above, but such aims are of enormous importance to the industry. We, therefore, for this class of student, restrict admission to the workshops to those who are actually engaged in optical workshop work during the day, and thus ensure that the lessons that are being impressed upon them at the Institute have some chance of

being applied in their daily life between successive attendances.

The optical trade, however, is peculiar, and differs from some other trades in this matter, in that it is desirable that students who are not intending to become actual workers at the bench in the industry should acquire some little knowledge of workshop practice, and that they should be able to check and verify their theoretical calculations and design work by actually making some of the parts of the optical apparatus to which these calculations and designs refer. A knowledge of the processes of grinding and polishing is of use to these students, because in many cases it is easier to modify an existing lens in a definite direction rather than to attempt to secure the specified curves exactly. The figuring of lenses is also a very important operation for such students. When we call to mind the many distinguished constructors of optical instruments in the past, for instance, the Herschels, Lord Rosse, Dr. Commins, and many others, who, commencing as amateurs, have materially advanced the practical methods of the industry, we see how fruitful a working knowledge of workshop methods may prove to optical students.

Not only, however, does our experience show that the more advanced students can thus obtain distinct benefit from attendance in the workshop, but also that students in an intermediate or second year's course can realise more effectively the value of their laboratory work by going through a specially arranged workshop course.

With respect to the drawing office work I do not propose to detain my hearers long, though the importance of this work must not be underrated. It is obvious that the making of working drawings to scale is one of the best methods of training the eye to read such drawings when produced by others, and the workman or other worker who cannot create a mental picture of the complete instrument from drawings in plan, elevation, and section is very much handicapped in his work. Moreover, the minute study of detail involved in making working drawings cannot fail to develop a "constructive eye" in the draughtsman which will save him from making absurd mistakes in the disposition of his materials. In addition, the power of producing such drawings is of

great value, as they form the *lingua franca* of constructional work between the workers in any industry. Looseness in drawing usually indicates looseness in thinking, and inevitably leads to bad work. It may be of interest to note in passing that the plan of the drawing office is that as soon as the student has attained some facility in the use of his drawing instruments he is set to work upon drawings directly connected with the industry in which he is interested. For instance, we would not set an optical student to draw the slide valve of a steam engine, but would let him have examples for practice drawn from the optical industry, thus insensibly accustoming him to the appearance of the ordinary instruments as they are depicted in working drawings to the correct proportioning of the various parts and to the other numerous details which make for good design. In this way the student becomes familiarised, through the eye, with much which can be best taught by such methods, and his study of the principles of design to be taken up probably later is greatly facilitated.

I cannot conclude these remarks on technical instruction in optics without a few words specially addressed to our students' section on the importance of a diligent study of mathematics as an absolute essential to the attainment of facility in dealing with optical problems. Only too well do I know, as the result of long years of experience of students of applied science, that this is not only the least attractive section of the work which we ask the students to face, but that in the great majority of cases there is a positive disinclination to tackle the subject at all, coupled with a readiness to drop it on the slightest pretext. For instance, only last Saturday I received a letter from an optical student who had been asked to explain his absence from the "Calculations" class, his excuse being that he had given it up because it was not required in the S.M.C. examinations. No greater mistake could be made. It is true that mathematics is only required as a specific subject in the "special examination," from which many candidates are exempted, but for a student, therefore, to neglect it is, to put it at its lowest, to handicap himself considerably in the race for success. The excuse, however, is a common one, and I would suggest to the authorities that they should seriously consider the desirability of setting a

paper in elementary mathematics as an obligatory part of the examination for every student.

But whence comes this rooted dislike to mathematics, a dislike which is so widespread amongst the students of our technical institutes? Doubtless it is partly because a somewhat greater intellectual effort is required from the students by this subject, that its utility is not so apparent, and still more that the results obtained are not so attractive to the ordinary mind. But these are far from being the only explanations, and from careful observation I am convinced that much of the disinclination referred to is due to the defective methods of teaching the subject in our elementary and secondary schools. (Hear, hear.) Take, for instance, the teaching of decimals. Could anything be more calculated to create a dislike of this commonsense method of calculation than to place it high up in the arithmetic after a long course, of no obvious utility, in vulgar fractions usually of a highly complex type? The natural place for its introduction is in the numeration table in the forefront of the four simple rules. So placed it would lose all its terrors, and instead of being regarded, as it now is by the majority of students, as something terribly difficult and almost sacrosanct, it would fall into its natural place, and decimals would be used as a matter of course in all calculations, with a very few exceptions.

Similar remarks could be made with regard to the teaching in the schools of elementary algebra and elementary geometry, but I must not weary my hearers by going into details in a subject in which perhaps I am somewhat of an enthusiast. Suffice it to say, and I say it with due deliberation, and as the result of wide experience, that these subjects can be so taught in their elementary stages as to leave on the minds of the majority of students an impression of attractiveness, rather than one of repulsion such as is nowadays too often produced.

I had intended to have dealt in some detail with the simplified methods of calculation, which, during the past twenty or twenty-five years have been developed for the use of the optical worker. I refer chiefly to curvature methods of dealing with optical problems, and in connection with these it must be a matter of gratification to this Society to note that in their development, a lead-

ing, indeed, a dominant, part has been taken by two of its past-presidents, Dr. Thompson and Dr. Drysdale. In the science of optics nothing is easier to one who has some mathematical ability, than to pile Ossa on Pelion in page after page of almost endless mathematical formulæ, whose chief effect is to increase the bewilderment of the student and to obscure the physical facts underlying the various stages of the investigation. It must not be supposed that I attach no value to intricate mathematical analysis, or that I regard it as being, in all cases, unnecessary. On the contrary, there are some problems, but, fortunately, they are not of very common occurrence, which can only be handled in this way. Hence the incentive of the student to perfect his mathematical knowledge, at least, sufficiently to enable him to understand, if not to originate, such investigations. My object, at the moment, is to reassure him and to press upon him the view that although such high mathematical ability should be cultivated wherever time and opportunity allow, the simplified methods to which I have alluded above have placed within reach the solution of problems which formerly could only be handled with the aid of high mathematics. All the more obligation then is there upon the average optical student to obtain a thorough grounding in elementary mathematics so as to enable him to take the fullest advantage of the new methods.

Although time and a desire not to try too much the patience of my audience prevent me from going as fully as I had proposed into details, I hope my words will not fall unheeded, but that the students may be influenced thereby to pay increased attention to their mathematical studies.

In conclusion, I hope that I have not wearied my audience with the technical details to which I have referred. My reason for going so fully into these details of optical education is, that at the present moment we are, I hope, approaching the end of a transition period, and when the full development comes and optical education in this country begins to be understood from the proper point of view it may be useful to those who are working at the question to have on record the experience which I have embodied in this paper.

With cordial thanks to my hearers for their patience

in listening to me so long, I must now address myself to the work which their confidence has placed upon me for the ensuing year.

APPENDIX.

Summary of the Experiments in the Technical Optics Laboratories of the Northampton Institute, London, E.C.

I.—Preliminary Optics Course

(*Experiments Quantitative wherever possible*).

(A) EASY MEASUREMENTS:—1-4.—Reading of scales, use of verniers, and measuring screws, measurement of angles.

(B) REFLECTION:—5-8.—Reflection at plane surfaces, formation of image, change of path of light, verification of laws.

(C) CURVATURE:—9.—Curvature of a concave reflecting surface, the diopter; 10-11.—Use of the spherometer and the Geneva lens measure.

(D) CONCAVE MIRRORS:—12-14.—Power by (*a*) parallel incident light, (*b*) the telescope method, (*c*) conjugate foci; 15.—Magnification.

(E) CONVEX LENSES:—16-18.—Power by (*a*) the telescope method, (*b*) conjugate foci, (*c*) neutralisation; 19.—Magnification; 20.—Tracing paths of waves through half-lens.

(F) NEGATIVE LENSES AND MIRRORS:—21-22.—Power of concave lens (using a convex lens) by (*a*) the telescope method, (*b*) conjugate foci; 23.—Power of convex mirror by the ophthalmometer method; 24.—Curvature of convex mirror by the parallax method.

(G) REFRACTION:—25.—Path of light through a block of glass; 26.—Refractive index of (*a*) glass plate, (*b*) water by microscope method; 27.—Deviation produced by prisms of small angle; 28.—Approximate deviation produced by prisms of large angle; 29.—Measurement of angle of a prism; 30.—Minimum deviation of prism of large angle; 31.—Refractive index of glass by experiments on prisms; 32.—Combinations of prisms, neutralisation; 33.—Deviation and base-apex lines of prisms;

34.—Prismatic effect of decentring lenses; 35.—Critical angle of a liquid.

(H) CYLINDRICAL LENSES:—36-38.—Power and axes of cylindrical and sphero-cylindrical lenses by (*a*) conjugate foci, (*b*) telescope method, (*c*) neutralisation; 39.—Testing centring of sphero-cylindrical lenses.

2—Applied Optics Course.

(A) EXAMINATION OF INSTRUMENTS:—

(i.) THE CAMERA.—40.—The pin-hole camera; 41.—Focal length of camera o.g.; 42.—Use of stops and their values.

(ii.) THE MICROSCOPE.—43-44.—Magnification of microscope by (*a*) double vision, (*b*) camera lucida.

(iii.) THE SEXTANT.—45.—Adjustment and use of.

(iv.) OPHTHALMIC INSTRUMENTS.—46.—The ophthalmoscope model and instrument; 47-49.—The ophthalmometer (*a*) model, (*b*) ordinary, (*c*) Helmholtz form.

(v.) THE TELESCOPE.—50-51.—Magnification by (*a*) double vision, (*b*) dynameter..

(B) MORE ADVANCED MEASUREMENTS:—

(i.) LENSES AND COMBINATIONS OF LENSES.—52.—Curvature of small lenses; 53.—Combinations of lenses on contact; 54.—Cylindrical and sphero-cylindrical lenses; 55-56.—Nodal points of separated lenses (*a*) telescope method, (*b*) collimator method; 57.—Back foci of separated lenses; 58.—Nodal points of combinations; 59-62.—Equivalent focal length of combinations by (*a*) two collimators, (*b*) one magnification, (*c*) two magnifications, (*d*) Blakesley's method; 63.—Effect on power of combination by altering relative position of lenses; 64.—Centring of lenses.

(ii.) ABERRATIONS.—65.—Chromatic and spherical aberration of a single lens; 66.—Astigmatism and curvature of field of single lenses, advantage of periscopic form.

(iii.) MATERIALS.—67.—Accurate measurement of refractive index; 68.—Dispersion; 69.—Wave-length of light in air; 70.—Testing pebble.

(C) ELEMENTARY POLARISCOPE EXPERIMENTS:—71.—Examination of crystals; 72.—Testing for strain in glass; 73.—Estimation of thickness of selenite films.

(D) INTERFERENCE:—74.—Testing of surfaces by Newton's rings.

3.—Examination of Optical Instruments.

(A) CAMERA:—75.—Measurement of focal length and aperture; 76.—Speed of shutters; 77.—Magnification and aperture in telephoto combinations; 78.—Testing dark slides and dark-room lamps; 79.—Testing view finder; 80.—Photographic method of testing lens; 81.—Testing for flare.

(B) TELESCOPE:—82-83.—Magnification by dynameter and camera lucida methods; 84.—Focal lengths of objective and eye-piece; 85.—Squaring on of o.g.; 86.—Simple tests of definition.

(C) LANTERN:—87.—Testing of objective and condenser; 88.—Evaluation of effective aperture of projection lens; 89.—Photometry of luminant.

(D) MICROSCOPE:—90.—Measurement of magnification; 91.—Focal lengths of o.g. and e.p.; 92.—Measurement of N.A. of o.g.; 93.—Mercury globule test of microscope.

(E) SURVEYING INSTRUMENTS:—94.—Evaluation of stadia lines; 95.—Testing of bubbles.

4.—Use of Optical Instruments.

(A) CAMERA:—96.—Use of shutters; 97.—Use of rising and tilting fronts.

(B) TELESCOPE:—98.—Use of 3″ and 5″ equatorially mounted refractors; 99.—Use of 8¼″ equatorially mounted reflector; 100.—Use of small transit instrument with chronograph.

(C) LANTERN:—101.—Adjustment and use of lantern.

(D) MICROSCOPE:—102.—Use of microscope; 103.—Centring of condenser; 104.—Preparation and staining of physiological specimens; 105.—Preparation of crystal sections, use of micro-polariscope; 106.—Use of selenite and quartz wedge; 107.—Drawing camera; 108.—Various methods of illuminating; 109.—Micro-photography.

(E) SURVEYING INSTRUMENTS:—110-115.—Adjustment and use of (*a*) ordinary and (*b*) prismatic compass, (*c*) sextant, (*d*) dumpy and Y level, (*e*) transit theodolite, and (*f*) miner's dial.

(F) SPECTROSCOPES:—116-117.—Adjustment and use of (*a*) spectrometer, (*b*) direct vision spectroscope.

(G) MEASURING INSTRUMENTS:—118-121.—Use of (*a*)

chronograph, (*b*) micrometers, (*c*) measuring microscope, (*d*) simple form of cathetometer.

5.—Physiological Optics.

(A) The Construction and Use of the Following Instruments:—122.—Optometer; 123.—Ophthalmoscope; 124.—Ophthalmometer; 125.—Perimeter; 126.—Riseley's rotating prism; 127.—Amblyoscope.

(B) Examination of Models:—128.—Special artificial eyes; 129.—Perrin's eye; 130.—Frost's eye.

(C) The Eye:—131.—Measurement of refraction of eye; 132.—Measurement of accommodation and convergence.

6.—Advanced Physiological Optics.

133.—Dissection of sheep's eye; 134.—Microscopic examination of eye-sections; 135.—Preparation of microscopic specimens; 136.—Ophthalmometry; 137.—Perimetry; 138.—Phakoscope; 139.—Illusions; 140.—Colour vision; 141.—Binocular vision; 142.—Entoptic phenomena; 143.—Optical properties of eye; 144.—Centre of rotation.

7.—Design of Optical Instruments.

(A) First Order Measurements:—145.—Equivalent focal power and back focus of combinations, Blakesley's method; 146.—The Fokometer method of measuring focal power; 147-148.—Equivalent aperture (Kew method, Drysdale's method).

(B) Materials:—149-154.—Spectrometry by (*a*) ordinary spectrometer, (*b*) Abbé spectrometer, (*c*) modification of Abbé method, (*d*) Chalmers' method for lenses, (*e*) critical angle method, (*f*) microscope method (workshop test); 155.—Verification of angle of prism (Chalmers and Ryland); 156.—Strain and striæ testing; 157-161.—Curvature measurement by (*a*) test plates, (*b*) Abbé spherometer, (*c*) Shaw spherometer, (*d*) ophthalmometer method, (*e*) Drysdale's method.

(C) Aberrations:—162.—Examination of lenses designed to illustrate various defects; 163.—The use of Hartmann method to show defects of simple lenses.

(D) Telescopes:—

(i.) Object Glasses.—164-167.—Standard methods of testing (*a*) collimator with both types of artificial

star, (*b*) auto-collimation method, (*c*) Hartmann method, (*d*) knife-edge method.

(ii.) EYE-PIECES.—168-169.—Focal power by (*a*) special apparatus, (*b*) Blakesley's method; 170.—Determination of chromatic correction of o.g. to use with e.p.; 171.—Special forms of e.p., illustrating modification of form of Ramsden and Huyghen's e.p.; 174.—Magnification by dynameter method; 175-176.—Parallelism of axes of binoculars and absorption by binoculars.

(E) PHOTOGRAPHIC LENSES:—177-181.—Testing on Beck bench, (*a*) ordinary tests for near and distant objects, (*b*) special swinging test, (*c*) Hartmann test, (*d*) auto-collimating test, (*e*) photographic testing; 182.—Special camera for photographic testing.

(F) MICROSCOPES:—183-187.—Measurement of constants as in course 4 E (above). (ii.) TESTING OF OBJECTIVES.—The following is a good introduction to this most difficult subject:—188-191.—(*a*) Standard test objects, (*b*) mercury globule test, (*c*) Abbé test plate, (*d*) auto-collimation method.

(G) DIFFRACTION:—192.—Measurement of star disc; 193.—Evaluation of resolving power of microscope o.g.; 194.—Measurement of wave length by grating; 195.—Estimation of slit width by diffraction bands.

MEASUREMENT OF ABERRATIONS OF PHOTOGRAPHIC LENSES.

By S. D. Chalmers, M.A.

Read before the Optical Society, March 29th, 1906.

This paper deals with the measurement of the aberrations of photographic lenses from the point of view of the designer, and so will not deal with such methods as give the performance of the lens without furnishing numerical estimates of its aberrations.

In the first place it is important to be able to compare the actual performance of a lens with the numerical measure of its aberrations in order to obtain the limits of error permissible in the various aberrations when calculating a system. Again, the verification of the aberrations of a system by trigonometrical calculation is very laborious, and may frequently be avoided if it is possible to make the lens and measure its aberrations.

It is thus evident that there are considerable advantages in measuring the aberrations of a photographic lens, provided the results can be so expressed as to give the same results as would be given by calculation, and it be possible to deduce aberrations at intermediate points of field, or aperture, from the actual measurements.

The theory of aberrations shows that to the first approximation the performance of a system may be represented (omitting chromatic effects) by five aberrations, generally described as spherical aberrations, coma, astigmatism, curvature of field, and distortion. These aberrations may be expressed as absolute or relative aberrations, the latter being pure numbers indicating the correction of the system. For a detailed discussion of the expressions of these aberrations and their relation to

one another, a paper by Dr. Drysdale, at the Optical Convention†, may be referred to.

For our present purpose it is important to note that of these first order aberrations, the spherical aberration varies as A^3, the coma as $A^2 \tan \theta$, the astigmatism and curvature of field as $A \tan^2 \theta$, and the distortion as $\tan^3 \theta$, where A represents the aperture ratio and θ the semi-angle of field considered.

But though the aberrations of the single lens may be regarded as approximately equal to the first order aberrations and so be analysed in the way indicated above, it must be remembered that in practical systems the first order aberrations have been approximately balanced, while those of the second order have been left to take care of themselves, with the result that, in most practical systems, the second order aberrations bear a very much larger ratio to the first order than they do in single lenses, and that a small change in the system will have a considerable effect on the first order aberrations, and very little on the second.

Thus, although the second order aberrations are not proportional to the aperture and field in the same way as those of the first order, it is frequently convenient to represent them in the same way, indicating them by the first order aberration which would produce the same effect at full aperture or full angular field, because the best correction of the second order aberrations is generally produced by leaving a first order aberration of this amount, but of opposite sign, to balance the effects of the second order aberration.

But, while this representation is convenient for many purposes, it is frequently very important to be able to obtain the "run" of the aberration throughout field or aperture as well as the amount of its greatest value. Two methods of representation have been proposed—both graphical. In the first, the aberrations expressed as longitudinal or lateral aberrations are represented by the ordinates of a curve, as in von Rohr "Die Photographischen Objective," where the spherical aberration and the astigmatism and curvature of field, obtained by calculation, as longitudinal aberrations, are so repre-

† Dr. C. V. Drysdale, Measurement of Aberrations. Proc. Optical Convention, 1905.

sented. In the other method, described before the Optical Convention‡, the lateral aberrations for certain chosen rays obtained by calculation or measurement, are indicated by the distance between two points, the position of the ray as regards the axis being indicated by the position of one of these points (or, where aberrations are large, the mean of the two points). This method has the advantage that any defect due to the combination of two aberrations is apparent, while the effect of each portion of the aperture for any chosen field can be seen from the diagram, whereas, in the method generally used the astigmatism and curvature must be calculated for a specified aperture, and change of aperture will frequently affect the form of the curve.

In using these graphical representations it is necessary to arrange the methods of measurement so that the results are readily expressible in the desired form.

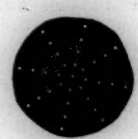

Fig. 1.

For practical measurements there are three principal methods depending on—

(1) The measurement of the size of the image formed on the focal plane, accompanied by some estimate of the distribution of light in this image.

(2) The determination of the positions of best focus for suitable objects.

(3) The evaluation of the positions in which specified sample rays cross the focal plane or any plane parallel to it.

The first of these methods gives most information as to the actual performance of the system under the given conditions. But it is very difficult to differentiate between good lenses, or to obtain a numerical estimate of the performance of the lens. It is quite unsuited for

‡ S. D. Chalmers, Aberrations. Proc. Optical Convention, 1905.

testing the performance of partial systems not completely corrected.

Again, it is practically impossible to determine the cause of given defects and so furnish the information the designer wants, while the choice of focus depends on the personal equation of the observer, and the measurements give no means of estimating the effects of an alteration of focus.

The second method avoids this last difficulty; it gives some information to the designer as to the cause of errors, but it does not give, except under unjustifiable assumptions, a means of obtaining the actual performance. It is defective because there is in general no definite sharply defined focus even for a series of parallel lines, and it is frequently possible to choose two positions

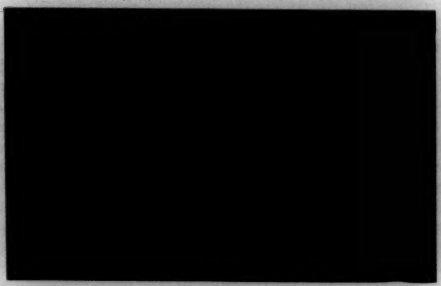

FIG. 2.

of focus so far apart that one estimate of the aberration is twice the other. This may be specially marked for large apertures at moderate angles of field.

In both methods the results, when obtained, are suitable for expression as curves; in the first the lateral, in the second the longitudinal aberrations being the ordinates of the curves.

In the third method the performance of the system is not given directly, and it can only be deduced from the aberrations as measured, but it can be made to show first order and second order aberrations

separately; it shows the effects of combined aberrations of the second order. It gives the coma both of first and second orders with an accuracy unattainable by other methods. It is suitable even for totally uncorrected systems, and its results are at once expressible in the diagrammatic form indicated above.

It now remains to describe the actual methods employed in obtaining the measurements according to each of these principles, and the apparatus and methods in use at the Northampton Institute will be taken as convenient illustrations.

Testing Methods in use at the Northampton Institute.—For all measurements the Beck lens testing bench is used. When the lens is to be used for near objects the standard test of the bench* is used, but for lenses to be tested on distant objects we employ the special swinging test, in which the image is examined on the focal plane or the small departures of the best focus from that plane are directly measured.

It may be interesting to see how this test was devised. When the lens-testing equipment of the Northampton Institute was being specified it occurred to me that the troublesome reductions of the ordinary tests might be avoided if the measurements could be made from the proper point on the focal plane and not from a fixed point. The original intention was to place a test object on the focal plane and to examine this object by the lens and a telescope, observing the focusing motion, either in the telescope or lens, necessary to obtain best general focus, or the best focus for special lines of the test object. But difficulties of illuminating the object evenly and satisfactorily caused Mr. Horace Beck to reverse the test by placing a microscope on the bar in such a way that it remained focused on the focal plane of the lens when the lens was rotated about a vertical axis. The convenience of this test is so great that it has become the standard test for lenses which are used for distant objects.

In using this test the lens is screwed into the lens holder, the observing microscope is brought to a standard position, the plate carrier racked till the image of the

* Photographic Lenses. Beck and Andrews. Appendix.

distant object is sharply focused. The lens holder is swung about the vertical axis and the whole of the carrier racked till the image comes back to its original position in the field of the microscope. On swinging the lens the image should be practically stationary, and a very slight adjustment enables the nodal point to be set exactly above the axis of rotation. The distance from the centre of rotation to the plate carrier can be read off on a suitable scale and gives the equivalent focal length.

Spherical Aberration.—To measure the spherical aberration the focusing positions for different zones may be measured by the aid of a set of patch stops, but it is more convenient to measure the diameter of a patch of light which is obtained when a small circular aperture of known dimensions is used as source (where light is a consideration the aperture may be of considerable size and the difference between the theoretical and observed size noticed). For this and similar measurements the microscope is provided with a micrometer eye-piece reading to .1 mm., and corresponding to 1/150 mm. with the objective usually employed.

Coma.—This aberration is shown by a one-sided appearance of the image, but it is not possible, in the ordinary way, to obtain a numerical estimate of the coma, because the figure seen is invariably complicated by the presence of astigmatism and curvature of field, and the complicated effects of combinations of these aberrations render analysis difficult, and the use of objects giving finite images would still further complicate the reductions.

Astigmatism and Curvature of Field.—If there were no coma (as is the case in most double symmetrical systems), the astigmatism and curvature of field could be deduced from measurements of the diameters of the elliptic patch on the plate, or from the measurements of the best focusing positions for horizontal and vertical lines. But in practice there is considerable latitude in the choice of the position of best focus, because the presence of spherical aberration at any angle causes portions of the aperture to give good foci in different places. In spite of these difficulties, however, it is possible to obtain sufficiently accurate results for systems of the same type, to enable us to compare the performance of an individual member with the standard by either of

these methods. Thus, for manufacturing purposes it is unnecessary to use more elaborate tests, except for isolated samples.

DISTORTION is most conveniently estimated by the actual movement, in the field of the microscope, of the image when the lens is swung. (This presumes great accuracy in the apparatus.)

THE HARTMANN TEST.—This method, which was devised by Professor Hartmann, has been described by him in the 'Zeitschrift für Instrumentenkunde,'† and by the author in the Proceedings of this Society and elsewhere.‡

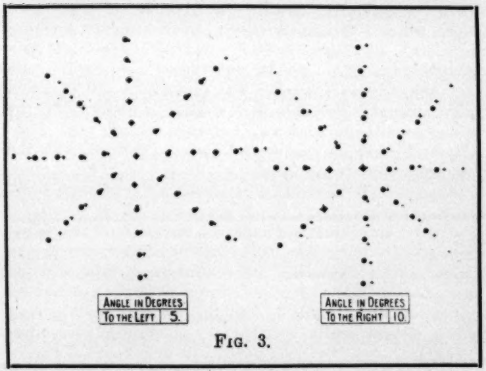

FIG. 3.

The author's modifications of this method have been directed to the simplification of the measurements of the photographs taken in accordance with Hartmann's method, and the expression of the results in diagrammatic form. In order that this diagram may be suitable for the deduction of the first and second order aberrations as separate quantities it is necessary that the diaphragm should be reasonably well centred and symmetrical about

† 'Zeitschrift für Instrumentenkunde,' translated by the OPTICIAN AND PHOTOGRAPHIC TRADES REVIEW.

‡ Proc. Optical Society, 1904. Journal of the Royal Photographic Society, March, 1905.

the axis, and should be in the plane of the effective stop. For lenses to be tested on distant objects a collimator is used and the lens rotated about a vertical axis, which is

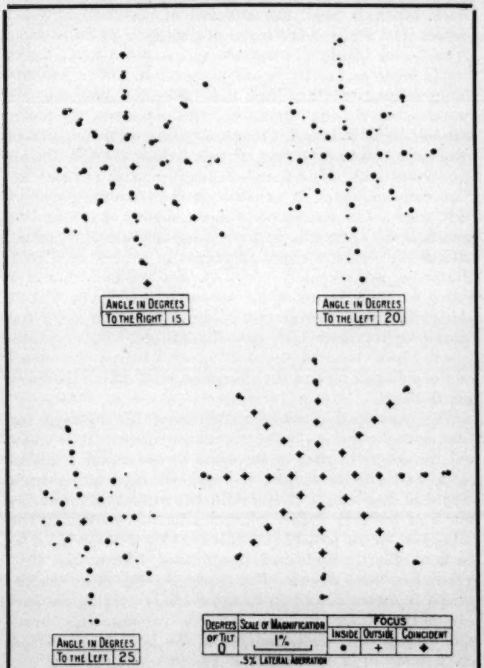

FIG. 8—(*Continued.*)

for convenience made to coincide with the nodal point of the lens. (Where the collimator is in any way defective it would be preferable to arrange to rotate the lens about a vertical axis passing through the "entrance

pupil" of the lens, *i.e.*, the image of the stop in the front medium—in double symmetrical systems the front nodal point.)

In ordinary practice the swinging test fixture of the Beck bench is used, the position of best central focus determined and the two series of photographs taken from 2 to 3 cm. inside and outside focus—the two distances being made as exactly equal as possible. When an arc lamp is used to illuminate the collimator aperture, approximately 1 mm. diameter, the exposures necessary are less than 1 second, though a considerable amount of over-exposure is desirable, as this brings out the diffraction rings and makes the photographs easier to measure. The stop employed is usually made of brown paper or thin metal, the apertures being stamped; it is usual to arrange the apertures in four lines, horizontal, vertical, and at 45°, giving eight apertures in a ring of 6 mm. diameter, and as many rings as possible, each diameter being 6 mm. greater than the one preceding.* Fig. 1 shows the contact print of an actual stop, and fig. 2 the photographs taken with this diaphragm. These photographs have been enlarged 5 times, to show the detail of the photograph and the sharpness with which the spots are defined.

The photographs so obtained must be enlarged for comparison purposes, but it is found convenient to draw out the centre of each of the spots by the aid of a microscope and camera lucida, the magnification being from 30 to 50 diameters; it is found convenient to obtain as much as possible of this magnification by projecting the image as far as possible, as this enables a larger field to be conveniently seen and illuminated. When one diagram has been drawn off completely, the other photograph is reversed, and its image made to overlap the corresponding diagram; the distance between the corresponding points, then, represents the lateral aberration on a scale of 2 m.: 1 or, if the two photographs are differently magnified, *i.e.*, a focus different to that selected visually decided upon, $m_1 + m_2 : 1$. Fig. 3 is a complete diagram for a good anastigmat in which the original magnifications were each 35, and so reduced

* For smaller lenses 5 mm. or even 4 mm. intervals are preferable.

that the lateral aberrations are represented on the scale of 24: 1.

These illustrations show that the aberrations are diagrammatically represented as described above; the cutting off is shown and the aperture ratio to which the various rings correspond given by their diameter divided by m times the distance between the photographs.

In the central photographs the two diagrams coincide within the limits of error in drawing, but at 10° a considerable amount of coma is noticeable, while a small amount of astigmatism can be seen; the exact amounts are difficult to measure because of the comparatively small magnification of the drawing in this special case.

In order to express the aberrations in the form of curves, the astigmatism can be deduced as a lateral aberration by considering the outside apertures for the vertical and horizontal lines, the sum of the aberrations for the vertical gives the vertical diameter of the image of a point, while the horizontal give the horizontal diameter, and from these two quantities the astigmatism and curvature for the aperture and field considered may be deduced. It is, however, important to note that for any special aperture the effect of spherical aberration and of curvature of field are identical, and it is necessary to consider either different angles of field or different apertures before these effects can be differentiated.

It is important to note that any difficulties of this kind will apply to the results of calculation (by trigonometrical methods) equally with the measurement results.

Thus, this method of measurement and representation makes the results strictly comparable with the results of trigonometrical calculations, and give all the desired information about the lenses.

PRODUCTION OF COLOUR BY COLOURLESS MEDIA.*

PAPER BY R. C. CANN-LIPPINCOTT.

Read before the Optical Society.

VERY few of those present here this evening are insensible to the impressions produced by colours. The colours of sunrise and sunset, the blue of the sky, the various hues of the rainbow, attract the attention of almost everyone. Struck by the beauty of these appearances of colour, some of you may have been led to enquire how they are produced. In answer to your enquiry, you will probably have been told:—(1) That all the colours are contained in light. (2) That light is the rapid undulatory movement of a highly attenuated hypothetical medium called the ether, which has been endowed with most contradictory qualities, and which is supposed to pervade all space and the substance of all bodies.†

(3) You will further have been told that the different colours are produced by the different lengths of the undulations of this hypothetical medium. (4) That the particular colours of ordinary objects are due to the fact, that these objects have stopped and absorbed all the undulations of the ether except those, which produce the particular colours, which distinguish these objects, and are reflected by them. For example, a red coat is red, because it absorbs all the colours except red. This it reflects. A blue flower is blue, because it reflects blue, absorbing the other colours.‡

The hypothesis that all the colours are contained in

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* From Goethe's work: "On the Theory of Colours." Paper read at the Optical Society.

† See "Light," by Lewis Wright, pp. 353-4.

‡ See "Six Lectures on Light," by Tyndall, Lecture I., p. 34.

light has always met with opponents, ever since it was first enunciated by Sir Isaac Newton. Lucas, Marriotte, Rizzetti, Castel, Gautier, Dollond, Klugel, and Goethe may be mentioned, among many others, as having opposed Newton's hypothesis. Nor have there been wanting opponents of the hypothesis of an invisible etherial medium, endowed with the most contradictory qualities, and supposed to pervade all space and the material of all bodies. Sir David Brewster, a great authority on optics, did not believe in it.§ Sir G. Grove, author of the "Correlation of the Physical Forces," considers light to be a force acting upon ordinary, but highly attenuated matter.‖

At the present time there appear to be differences of opinion as to the structure of the ether, even among those who believe in its existence. Professor Sir A. Rucker, in his address to the British Association, at Glasgow, in 1901, speaks of the mechanical theory of the atoms and ether as confessedly imperfect.* The Russian chemist, Professor Mendeleef, considers that the ether is the lightest and most attenuated form of matter known.† Some of you may have heard that another and a very different explanation of the production of colour has been given by the great German writer, Goethe, in his work "On the Theory of Colours" ('Zur Farbenlehre'). You will perhaps ask how came Goethe, a great poet, to turn his attention to so scientific a subject as the study of the physical colours?

Goethe was not only a great poet, he was also a lover of art in general; he was especially fond of painting, and had, since his childhood, spent much of his time in the society and in the studios of artists, and had himself made attempts at landscape painting. He was thus led to turn his attention to that important part of painting known as colouring. Neither from artists, nor from books could he, however, obtain any explanation or statement of the principles, which should guide the painter

§ "Six Lectures on Light," by Tyndall, Lecture II., p. 47.

‖ Quart Journal of Science, January, 1869, p. 3.

* Report of the British Association, 1901, p. 26.

† "Knowledge," May, 1904, p. 99. "The New Knowledge," by R. K. Duncan, 1905, p. 250.

in the colouring of a picture. Hence, he became convinced that colours must be studied first as physical appearances, and approached from the side of nature, if any mastery was to be gained over them in relation to art. Like the rest of the world he was convinced that all the colours were contained in light, he had never heard any other explanation of the production of colour given, and had never had the slightest reason to doubt that the ordinary explanation was correct, because he was not further interested in the subject.

Having decided to approach the study of colour from the side of physics, he read the chapter on the subject in one of the ordinary hand-books. Finding that he could not obtain the information he required from the theory as it was explained there, he determined himself to see the experiments, on which the received theory is based, and for this purpose borrowed some prisms.

It happened at the time that he was very much occupied with other matters, and, consequently, put off making the experiments with the prisms in the usual way.

The owner of the prisms sent for them, and Goethe was about to give them to the messenger when he thought that he would like to take a hurried glance through a prism. He found that white and uniformly coloured surfaces remained unaltered. An appearance of colour was only seen where a dark spot bordered on the white or other uniform colour of the surface. It did not require much thought to convince him that a boundary was necessary to produce colour, and he at once, instinctively as it were, exclaimed that Newton's theory was false. Returning the prisms was now not to be thought of, and Goethe persuaded their owner to let him keep them for some time longer.

Goethe next simplified the phenomena, which are to be seen in every day life through prisms in rooms, and in the open air, and reduced them to easy experiments by using simple black and white surfaces.

These experiments were published in a little treatise called "Contributions to Optics," in two parts. This was received unfavourably by the scientific men of the day.

Goethe did not allow himself to be discouraged by this, but continued to study and investigate the phenomena of colour for many years. In the year 1810 he pub-

lished his work on the "Theory of Colours," in two volumes, octavo, with a quarto volume of plates. This work is divided into three parts, the Didactic, the Polemical, and the Historical. The first part gives an "Outline of a Theory of Colours"; in the second part Newton's hypothesis is examined; the third part gives an account of what has been written on colours from the earliest times to those of the author.

The first, or didactic part, has been translated by Sir C. L. Eastlake.‡ The extracts from Goethe's work, which I propose to read to you this evening, are taken, with some additions and alterations, from this translation. In the didactic part, Goethe separates all the phenomena of colours into three divisions, according to the conditions under which they are produced, viz., the physiological, the physical, and the chemical colours.

1. The Physiological Colours.—They are the foundation of the whole doctrine, and open to our view the chromatic harmony, on which so much difference of opinion has existed. They may be said to belong to the eye itself, and to be produced by its action and reaction. They are fleeting, and not to be arrested. Examples will best explain what is meant by the term physiological colours.

If we look at a dazzling altogether colourless image, such as that of the sun at sunset, it makes a strong lasting expression, and its after-image is accompanied by an appearance of colour. (39.†) Again, let a small piece of bright coloured paper or silk stuff be held up before a moderately lighted white surface; let the observer look steadily on the small coloured object, and let it be taken away after a time, while his eye remains unmoved; the spectrum of another colour will then be visible on the white surface. The coloured paper may also be left in its place, while the eye is directed to another part of the white surface, the same coloured appearance will be visible there, too, for it arises from an image, which now belongs to the eye. (49.)

To the physiological colours are added, in an appen-

‡ Goethe's "Outline of a Theory of Colours," translated by Sir C. L. Eastlake.

† The numbers refer to the paragraphs in Goethe's work.

dix, the pathological. These give us a more complete insight into the physiological colours. So-called colour blindness is an example of the pathological colours. Persons who are thus affected see fewer colours than those whose eyesight is normal.

2. THE PHYSICAL COLOURS.—They are not produced by the eye acting in itself nor by the object. Not by the light acting directly on the eye. But indirectly by means of media, these media being themselves colourless. They are also transient, lasting only as long as the conditions, which produce them, continue.

3. THE CHEMICAL COLOURS.—Belong to particular substances, and may be made permanent for any length of time.

The physical colours may be sub-divided into:—(1) The catoptrical colours. Produced by reflection from the surfaces of media. (2) The paroptical. Produced when light shines past an edge and crosses itself. (3) The dioptrical. They are produced by colourless semi-transparent media. (4) The epoptical. Appear under various conditions on the surfaces of bodies, and between the surfaces of bodies. They also are transient, but under certain conditions become fixed and permanent. (5) The entoptical. Produced inside transparent bodies. They are generally known as the colours produced by the so-called polarisation of light.

DIOPTRICAL COLOURS.—The dioptrical colours are treated first, because they give the full impression of the physical colours, and exhibit their characteristics in a very striking way. They are divided into two classes, those of the first-class produced by colourless, more or less transparent, yet light transmitting media; and those of the second-class, which are exibited when the medium is in the highest degree transparent. (144.)

To see an object distance is required, some space must intervene between the eye and the object. No space is absolutely empty, hence no space can be absolutely transparent. We do not see objects directly, but through media. These media may be gaseous, fluid, or solid. The transparency of media is relative and depends upon the depth of the space which they occupy.

For example, air, water, and glass, are transparent up to a certain depth, at a great depth they are semi-transparent, while at a still greater depth they are only

light transmitting, till at last they become opaque.* Transparency itself, empirically considered, is already the first degree of the thickening medium (des Trüben). The intermediate degrees from this point to opaque white are infinite. (148.)

At whatever point short of opacity we arrest the thickening medium, it exhibits simple and remarkable phenomena, when placed in relation with light and darkness. (149.) The highest degree of light, such as that of the sun, of phosphorus burning in oxygen, is dazzling and colourless. So also the light of the fixed stars is for the most part colourless. This light, however, seen through a medium, but very slightly thickened, appears to us yellow. If the transparency of such a medium be decreased, or if its depth become greater, we shall see the light gradually assume a yellow-red colour, which at last deepens to a ruby-red. (151.)

If, on the contrary, darkness is seen through a semi-transparent medium (ein Trübes), which is itself illumined by a light striking on it, a blue colour appears, this becomes lighter and paler as the transparency of the medium is decreased, but, on the contrary, appears more and more dark and deep the more transparent the medium (das Trübe) becomes; in the least degree of dimness, short of absolute transparency, always supposing a perfectly colourless medium, this deep blue becomes sensible to the eye as the most beautiful violet. (152.)

If this effect takes place in the eye as here described, and may thus be pronounced to be subjective; it remains to convince ourselves still further of this by means of objective phenomena. For a light thus mitigated and passed through such a semi-transparent medium illumines all objects in like manner with a yellow, yellow-red, or pure red hue; and although the effect of darkness through the semi-transparent medium does not exhibit itself so powerfully, yet the blue sky displays itself in the camera obscura very distinctly on white paper, as well as every other material colour. (152.)

In examining the cases, in which this important funda-

* See Goethe's Works, Second Division, Vol. II., p. 226, Weimar edition.

mental phenomenon appears, we naturally mention the atmospheric colours first; most of these may be here introduced in order. (153.)

The sun, seen through a certain degree of vapour, appears with a yellow disc. The centre is often still dazzlingly yellow, when the edges are already red. The sun seen through a thick yellow fog (as was the case in 1794, even in the north) appears ruby-red; the same appearance is still more decided, owing to the state of the atmosphere, when the scirocco prevails in southern countries; the clouds generally surrounding the sun in the latter case are of the same colour, which is reflected again on all objects. (154.)

The red hues of morning and evening are due to the same cause. The sun is announced by a red light, in shining through a greater mass of vapours. The higher he rises, the brighter and yellower the light becomes.

If the darkness of infinite space is seen through atmospheric vapours illumined by the daylight, the blue colour appears. On high mountains the sky appears by day intensely blue, because only a few thin vapours float before the endless dark space; as soon as we descend in the valleys, the blue becomes lighter, till at last, in certain regions, and in consequence of increasing vapours, it altogether changes to a very pale blue. (155.)

The blue appearance at the lower part of the flame of a candle belongs to the same class of phenomena. If the flame is held before a white ground, no blue will be seen, but this colour will immediately appear, if the flame is held opposite to a black ground. This phenomenon may be exhibited most strikingly with a spoonful of lighted spirits of wine. We may thus consider the lower part of the flame as equivalent to a vapour, which, although infinitely thin, is, nevertheless, apparent before the dark surface: it is so thin, that one may easily see to read through it; on the other hand, the point of the flame, which conceals objects from our sight, is to be considered as a self-illuminating body. (159.)

Smoke, again, is also to be considered as a semi-transparent medium, which appears to us yellow or reddish before a light ground, but blue before a dark one. (160.)

If we now turn our attention to fluid mediums, we find, that any water, deprived in a very slight degree of its transparency, produces the same effect. (161.)

A few drops of scented water, of spirit of varnish, of many metallic solutions, may be employed to give various degrees of opacity to water for such experiments. Spirit of soap, perhaps, produces the best effect. (163.)

Among solid mediums the opal attracts our attention first in nature: its colours are at least partly to be explained by the circumstance that it is, in reality, a semi-transparent medium, through which sometimes light, sometimes dark, substrata are visible. (165.)

For all experiments, however, opal-glass (vitrum astroides, girasole) is the most desirable material. It is prepared in various ways, and its semi-opacity is produced by metallic oxides. . . . . (166.)

A striking experiment may be made in a dark room with sheets of parchment. If we fasten a piece of parchment before the opening in a window-shutter, when the sun is shining on it, the parchment will appear nearly white; by adding a second, a yellowish colour appears, which continues increasing as more and more leaves are gradually added, till at last it changes to red. (170.)

Having now traced the most splendid instances of atmospheric appearances, as well as other less striking, but still sufficiently important cases, to the leading examples of semi-transparent media; we have no doubt, that attentive observers of nature will carry such researches further, and accustom themselves to trace and explain the various appearances, which present themselves in every day experience, on the same principle; we may also hope, that investigators of nature will provide themselves with an adequate apparatus, in order to place such important results of experience before the eyes of others who may be desirous of information. (173.)

We venture, once for all, to call the leading appearance in question as generally described in the preceding pages, a fundamental and primordial phenomenon; and we may be here permitted at once to state, what we understand by the term. (174.)

The circumstances which come under our notice in ordinary experience are, for the most part, insulated cases, which, with some attention, admit of being classed under general empirical rubrics. These again range themselves under scientific rubrics, which are more comprehensive, and through which we become better acquainted with certain indispensable conditions of ap-

pearance in detail. From henceforth everything is gradually arranged under higher rules and laws, which, however, are not to be made intelligible by words and hypotheses to the understanding merely, but, at the same time, by real phenomena to the sight. We call these primordial phenomena, because nothing appreciable by the senses lies beyond them, on the contrary, they are perfectly fit to be considered as a fixed point, to which we first ascended, step by step, and from which we may in like manner descend to the commonest case of every day experience. Such a primordial phenomenon is that, which has lately engaged our attention. We see on the one side light, brightness, on the other darkness, obscurity: we bring the semi-transparent medium between the two, and from these contrasts, with the assistance of this medium, the colours develop themselves, contrasted in like manner, but soon, through a reciprocal relation, directly tending back to a point common to both.* (175.)

With this conviction we look upon the mistake which has been committed in the investigation of nature to be a very serious one, inasmuch as a secondary phenomenon has been placed higher in order, the primordial phenomenon has been degraded to an inferior place; nay, even the secondary phenomenon has again been inverted, and the compound effect in it has been treated as simple, the simple appearance as compound; owing to this inversion the strangest complication and perplexity have been introduced into physical inquiries, the effects of which are still apparent. (176.)

But when even such a primordial phenomenon is arrived at, the evil still is, that we refuse to recognise it as such, that we still seek for something further behind it and above it, although it would become us to confess that we have here arrived at the limits of observation. Let the observer of nature suffer primordial phenomena to remain undisturbed in their eternal peace and beauty;

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* That is (according to the author's statement, 150 and 151), both tend to red; the yellow deepening to orange as the comparatively dark medium is thickened before brightness; the blue deepening to violet as the light medium is thinned before darkness. (Eastlake's note.)

let the philosopher admit them into his department, and he will find, that a worthier material for further treatment and elaboration is afforded him in the fundamental and primordial phenomenon, than in insulated cases, general rubrics, opinions, and hypotheses. (177.)

To this primordial phenomenon Goethe traces the brilliant appearances of colour seen through lenses, prisms, and other refracting media, and the colours seen when light or dark images are passed through lenses or prisms on to surfaces opposite to them. To the same primordial phenomenon he traces the epoptic colours, or the colours produced by thin films, as, for example, the coloured circles known as Newton's rings, the colours of soap bubbles, of oil on water, and of varnish on aquafortis.

The colours, which spread like a breath over the surface of polished steel, and other metals when heated, and become actually fixed, are also to be deduced from the doctrine of the semi-transparent media. As these colours become fixed in bodies, we have arrived at the point where the chemical colours begin, nay, we have to a certain extent already passed these limits, inasmuch as when colours have become fixed in bodies, and belong to particular substances, they are classed as chemical.

The entoptic colours, generally known as the colours produced by the polarisation of light, were discovered after Goethe's work "On the Theory of Colours," had been published. Goethe wrote an essay on these colours which was published in the supplement to his work "On the Theory of Colours." In this essay he traces these colours also to the primordial phenomenon of light and darkness as affected or acted upon by colourless media of various degrees of transparency.

THEORIES OF ACCOMMODATION OF THE EYE.

PAPER BY HARRY L. TAYLOR.

Read before the Optical Society on May 17th, 1906.

I AM asking the favour of your consideration of a subject which is by no means new—it is "Theories of Accommodation of the Eye." Let me assure you that I have no new theory to put before you, nor do I propose to discuss the many which have, at one time or another, been given to the world. Nearly every great name in ophthalmology is connected with the subject, but three stand out prominently—our own immortal Young, Helmholtz and Tscherning.

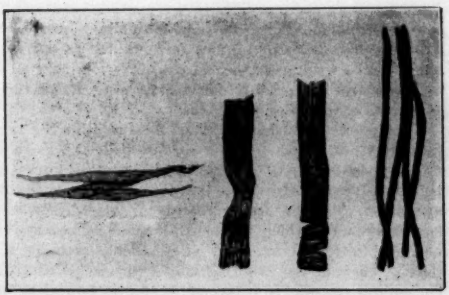
The mechanism of accommodation is as important as it is fascinating, and yet tantalising, for nearly every question of ophthalmology, and especially refraction, finds accommodation a factor to be considered. We know well what accommodation does; we are not so sure as to the mechanism of the act itself, and, consequently, in many instances we get two diametrically opposed explanations of some phenomena, because we have two theories of accommodation, especially, which are opposite and antagonistic. You will see that I refer to the theories of Helmholtz and Tscherning.

In estimating the value of one of these theories over another we give preference to that which tallies better with facts as we know them. We have to consider our sources of information for these facts, and we may group them as anatomical, physiological, and pathological (in which, for our present purpose, I include errors of refraction). Unfortunately, take whichever group we may, we find most of the problems presented may be explained by either theory. Anatomy should be our best friend, but the greatest controversies have raged over points of structure, and we are much handicapped because the eye changes so rapidly after removal from the socket.

Accommodation is a muscular act, the muscle causing it being of the involuntary or smooth fibred kind, and is part of a graduated thickened ring inside the sclerotic coat of the eye, being bedded within other tissues which form the greater part of the ring referred to. This occurs just behind the sclero-corneal junction, as seen in the diagram, where it is shown in section. That is to say, the muscular ring will come just round the eye. In the diagram (fig. 3) it is shown in section, the portion of the eye being cut across.

You see there the position of the muscle which is cut through. This diagram is purely diagrammatic for certain reasons, but it shows the position of the muscle.

Fig. 1.



Unstriped (1) and Striped (3) Muscular Fibres.

The muscle, then, generally speaking, is a ring inside the sclero-corneal junction. Here is a small diagram (fig. 1) showing the difference between the striped fibres of the muscle such as are found in the external muscle of the eye, and the unstriped fibres of which the ciliary muscle is composed. This muscle has been the cause of very many disputes.

The numbers of the groups of fibres vary according to different authorities, but we put it in this way, that, generally speaking, the fibres vary in arrangement from those disposed, in one position, to some which are disposed almost at right angles and form almost circular

bands. Some say that there are four groups of fibres, others that there are three and five, but it seems that, in a general way, one set graduates into the other.

The result of the contraction of this ciliary muscle is an alteration in shape of the crystalline lens, a transparent body with a varying optical density, and enveloped in a sort of inextensible bag known as the capsule. The lens, of course, is found just inside the eye at the back of a curtain—the iris. The lens is composed of a number of layers arranged much as in an onion, the disposition of constituent elements being identical in each layer, and the individual fibres are disposed in a kind of bent spring arrangement, so that compression on the sides adjoining an angle places a tension upon the whole, and the accumulated tensions on these fibres are a special feature of the act of accommodation. Released from pressure of any kind the lens takes its normal form, which is somewhat similar on its anterior face to the surface generated by an ellipse revolving on its short axis, while its posterior is more like that of a parabola.

This posterior surface is fitted behind into the semi-fluid vitreous, which forms a sort of pad, and is generally supposed to resist any great alteration of curvature on that face of the lens, the force which causes an alteration of shape acting mainly in the direction of least resistance, which is the front surface, because the aqueous, which is in front of the lens, is more like water, whereas the other is more like a jelly. The enveloping capsule is attached by fibres known as the zonule of Zinn to the surrounding circular plicated mass, and from the direction of these it is certain that any tension on them tends to pull the lens backward on to the pad of the vitreous.

It is evident, from this brief description, that tension on the capsule and the corresponding relaxation, are the great features in the act of accommodation, and there can be no doubt but that contraction of the ciliary muscle is the force which dominates these states. On this point there is complete agreement, but with regard to the effect of the muscular contractions upon the convexity of the lens, caused by tension and relaxation, there is absolute variance between the Helmholtzian school and the followers of Tscherning, for the former

maintains that tension on the capsule flattens the lens and reduces its convexity, while the latter insist that this same tension increases the convexity by the formation of a lenticonus. Thus, although the amount of the tension is the same upon either assumption, the effects are opposite.

According to Helmholtz, contraction of the ciliary muscle pulls forward the choroid and releases the zonule, thus allowing the lens to assume a more convex form due to its elasticity, but Tscherning maintains that this contraction puts a tension on the zonule in such a manner as to pull the anterior part of the capsule tense, and also to pull the choroid forward. He further believes that this traction on the membrane compresses the vitreous against the lens, and that the posterior part of the capsule relaxes in consequence, causing the hinder part of the lens to participate in the extra-convexity of accommodation to the extent of about four-tenths. Tscherning believes that the denser nucleus of the lens is responsible for the lenticonus, causing the softer peripheral parts to shape in accordance with its position.

The main points are these, that according to Helmholtz's theory, the tension of the choroid or dark coating, as we call it, pulls forward the coating a trifle, and because this ligament is attached ultimately to that coating, it follows that the ligament must be relaxed. The muscle pulling this part up must relax that, and so the lens becomes more convex, because of that elasticity from the bent spring arrangement. On the contrary, Tscherning's theory, mainly, is to the effect that by contraction of the muscle this is pulled tight, and, in being pulled tight, it will pull the lens flatter, naturally, but owing to the denser centre of the lens the outer parts seem to shape in accordance with that denser centre, and form what is known as lenticonus, thus giving an increased power.

The facts adduced in support of this explanation are exceedingly interesting, and carry great weight, because of the skill of the author, and particularly because they explain much which was obscure in the daily work of the refractionist in the determination of refractive error. For details concerning them I must refer you to his well known treatise on physiologic optics. I propose now to put before you some details of structure of the accom-

modative apparatus as investigated by Hocquard, some of which seem to me to have an importance far greater than is usually assigned to them, indeed, few authors appear to have found them worthy of consideration.

I will first ask your attention to the zonule. As illustrated here, you will see that it is divided into two parts. (1) An anterior outer portion from the lens margin to the head of the ciliary processes. (2) A posterior portion stretching from the inside face of the ciliary body right to its origin at the ora serrata. When the cornea and iris of an eye are dissected out, and it is put under water and viewed by a low power microscope, we see the clearly defined circular margin of the lens with a crowd of very fine cords coming from under the ciliary processes, and all directed towards the centre.

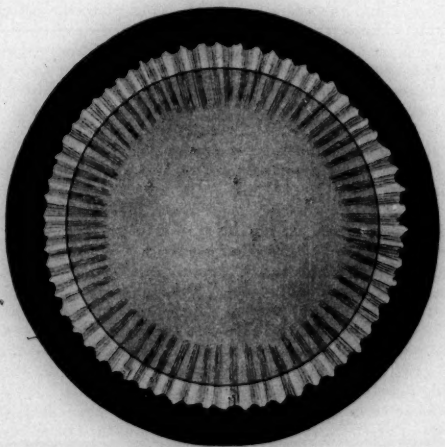
There is another diagram (fig. 2) which shows it fairly well on a very large scale, and this, by-the-bye, causes a curious optical illusion. In a good light, if you look at the centre of it, the thing seems to invert itself. The centre seems to be almost in motion, especially if you get the correct distance, I take it that the explanation of that is in the cords which run towards the centre, and probably there is some shading in the colour which leads the eye on towards the centre, and then the eye at once takes a different point of view.

This represents an eye looking down from the upper part after the cornea and iris are removed. These nobs at the outside are the heads of the ciliary processes—really they are these heads which you see in the section. These fine lines from them are some of the cords which I am about to describe. The circle is a margin of the lens, so that you see these lines cross the lens in a measure. Some disappear under the back surface of the lens, and others are fixed on to it just at the equator, while a third group is attached to the front surface (of the capsule). The turgescent processes seem to press on them as a finger on stretched cords. You will notice a curious resemblance in this diagram to this hammer-like body, almost as if it were a piano hammer. When these cords are broken by a needle, they break as glass would, but when one is bent back it re-dresses like a spring which is uncoiled. They are pliable, but inextensible, are all of nearly the same calibre, and do not anastomose

at all. Those attached to the equator are the least numerous of the three sets, and do not seem to be continuous round the lens, many authors omitting their description, while the posterior series are connected a little nearer the edge than those of the front surface.

Each cord of all sections, before coming into contact

Fig. 2.



Crystalline Lens-ciliary Processes and Zonular Bundles viewed from above.

with the capsule, divides into a brush of fine fibrilles which separate and have distinct junctions, the longest always being those which are fixed on furthest from the equatorial plane of the lens, but the thickest and closest set are those at the equator. You will see the longest fibres on the outside, and when we come to the equator we find them shorter, generally speaking, than any of those on the upper sides (fig. 3).

It is very difficult to examine the second part of the capsule—that from the ora serrata to the ciliary processes, for in any section the cords become piled together

and cannot be dissociated, as the part runs inward. But in a case of chronic glaucoma Hocquard obtained some microscopic sections of the ciliary region in which a kind of dropsy had separated the fibres in places.

At the ora serrata the choroid changes. It loses most of its vessels and becomes a fibrous layer, separating from the sclerotic to make room for the ciliary muscle. The ciliary muscle seems to be interposed between the choroid and the sclerotic. At the same place the retina rapidly diminishes in thickness and is only represented by a layer of clear cylindrical cells (pars ciliaris retinae) which continue right forward on to the anterior face of the processes. Just where the retina is transformed from a membrane to a cellular layer is a sort of slit into which the zonule disappears. Injections made into the anterior chamber find their way into this slit, and so constitute it into a diverticulum on the external face of the vitreous.

Across this space are found glassy, rigid streaks, all directed forward, they unite into stronger groups, and further on two or three adjacent ones become welded together and form the true zonular bundles. These continue in a meridional course. That is to say, they come straight towards the front of the eye, and with a favourable section can even be traced to the crystalline. Some, bending round the processes go to the front of the lens capsule, and others, always deeper, go to the rear, running between the hyaloid hollow and the zonule.

These cords receive numerous fine fibres from the clear cells of the ciliary body, and they join the cords just like the fine barbs of a feather on to the central shaft. They have a remarkable regularity—always from back to front and from without to within. Hocquard calls them "direct fibrilles of attachment," as distinct from "recurrent fibrilles of attachment," which are a series running perpendicularly to them (fig. 3), and which are found almost exclusively in the angle formed by the processes with the ciliary body. It is thus seen that they bind the zonule firmly to the processes.

In tearing away the zonule, pigmented lumps come with it, and the processes seem to adhere closer to it than to the subjacent tissue. Thus, traction is distributed over a large area, a very important matter with delicate membranes. And you can quite see that

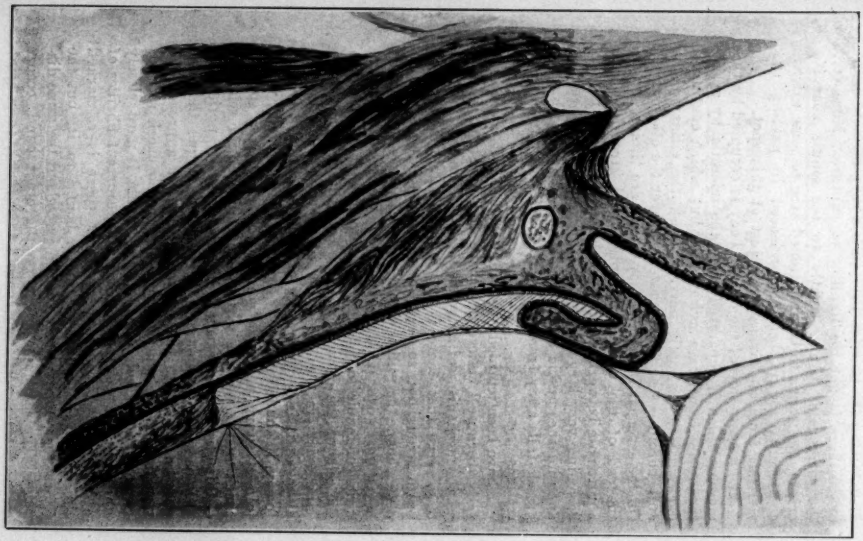


FIG. 3.—DIAGRAMMATIC SECTION SHOWING CILIARY MUSCLE AND ZONULAR ATTACHMENTS.

this seems a very reasonable sort of provision—that by distributing the traction over such a very delicate membrane there is not the liability to rupture. Moreover, the vitreous is bound tightly to the zonule near its origin, and seems to be partitioned here to some extent, a difficulty always being experienced in parting the vitreous from the tissues near just at this spot.

The longest side of the ciliary body is occupied by the muscle, which is separated from the sclerotic by a slit ending in front in a *cul de sac*, but which continues backward to the optic nerve. Thus, the ciliary body and choroid get a gliding movement over the other parts. On the external face these movements are limited up to the end of the choroid by pigmented strands which anastomose between the two at sharp angles, the meshes becoming very large near the ciliary and disappearing opposite the muscle almost entirely. This is why the muscle falls away from the sclerotic so readily in cutting sections.

I have been tempted to go into these details somewhat fully, because I believe they have an important bearing on the mechanism of accommodation. You will not fail to observe that there is a wonderful arrangement of parts throughout, so that when traction is exercised on a membrane the pull is a gradual one, and is spread out, as it were, over a large surface. We see this in the union of zonule to capsule, and also in that of zonule to the cells of the ciliary region. Something of the same kind exists in the apposition of choroid and sclerotic, although it is of the nature of a check action. That is to say, in the latter case it seems as if these cords, which join at sharp angles, are really intended as a sort of check on the movement of the choroid over the sclerotic —this gliding movement, which is generally noticed.

It is sound reasoning, therefore, to argue that the greatest movement takes place just as we approach the insertion of the muscle, for there are no check strands from it to the sclerotic, but the zonule is most closely united to the process by direct and recurrent fibrilles of attachment. That is to say, that there is a marked difference; the zonule is tightly bound to it, but the choroid is comparatively free, in just about the same district. The least movement, on the other hand, occurs where the strands, from choroid to sclerotic, form smal-

ler meshes, and where the choroid is united to the retina proper.

The facts, as ascertained by Hocquard, fit in exactly with the Helmholtzian theory, the traction on the choroid and the turgescence of the processes which are so intimately connected with the zonule point to a relaxation of tension on the capsule of the lens. If we are to agree with Tscherning we must suppose a simultaneous traction on choroid and capsule, and this combined with a turgescence of the processes seems scarcely probable from the peculiar connection of parts, and from a necessary alteration in shape of the ciliary muscle which is almost inconceivable from its position and form.

On the opposite side there are some remarkable facts which seem to support Tscherning's theory. His idea of capsular tension furnishes a beautiful illustration of adaptation. It has puzzled many to understand how the shape of a comparatively large mass such as the jelly-like vitreous could be maintained with a relaxation of its frontal retaining membrane as necessitated by the theory of Helmholtz. Tscherning's tension on the front of the capsule coincident with relaxation of the back, at the same time as the vitreous is compressed by the choroid provides for a retention of shape, and with accommodation relaxed, we should still get a support by the tightening of the posterior capsule.

There are many of Tscherning's observations, following the methods and work of Young, which any theory of accommodation must account for, as they are undoubtedly true. His investigations of the change in spherical aberration show that this decreases with extra dioptric power of the lens—a fact inconsistent with merely pure sphericity added, because we should, other things being equal, expect an increase. Again, using a form of Young's optometer, he found that during accommodation, the increase of refraction was greater at the centre than towards the margins of the lens. And, finally, by obtaining reflections of points of light from the surface of the lens he corroborated the same facts.

I want to call your attention specially to a form of Young's optometer which I have made, and this Young's optometer is, to my mind, one of the finest instruments that we have to-day in the determination of refractive

error. You can do almost anything with it, and you can find out almost any defect; in fact, I believe that you can find out more defects with that simple optometer than you can with any other instrument with which I am acquainted. The principle of it is this, that we take, we will say, an ordinary blank disc with four pin-holes, so close together, and in a perfectly straight line, that they will not extend beyond the margin of the pupil, that is to say, that you can see through them. You can see through all four.

Now, when you look through those four pin-holes at the piece of cotton, you see four pieces of cotton, only they will all come to a point. Those pieces of cotton, or the edges of this one piece, actually represent the course of rays from the point that you are looking at right through the cornea and on to the retina; the consequence is, that if you have those four holes at equal distance (or you can take five and yet get a central one—five is preferable), you can see just where all those rays meet, as it were, and it is a remarkable thing that practically no one will find those four images meet in the same spot; that is to say, that the two outer ones, or, if we take five, the two outer pairs, will cross at different distances. Now, supposing you had a length of cotton of about 20 yards, and looked through these pinholes at the end of it (that, for all intents and purposes, is the punctum remotum), you will find that by bringing the finger along it you can focus all the points in succession from that far point right up to your near point, and you can see the differences in the crossing of these lines from that far point to the near point.

Now, you will observe that those pin-holes, being in a straight line, of course, can be used in any meridian of the cornea. You can investigate every meridian. The consequence is that you find in investigating two meridians at right angles to one another, that you will get two different near points, and frequently two different far points. What does that mean? It is simply that you are finding your astigmatism, and you can estimate the amount of the astigmatism.

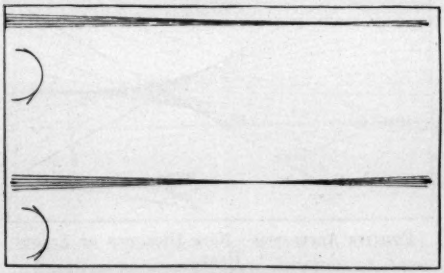
I might say, with this simple arrangement, that the difference between the two near points with the slit in one direction and then in the opposite direction, is what we might term the "interval of Sturm." That is,

the difference between the two focal lines, the one which would correspond to the meridian of greatest refraction and the other which would correspond to the meridian of least refraction.

Now, I have a series of diagrams which show this fairly well. The first one is the representation of two corneas, and in this case it shows the curve or arc of the circle, with the cornea drawn just above showing the departure from the arc of the circle; it is only illustrative, but I want you to notice that on the one side the curve is not quite the same as on the other side of that arc, and you see a difference then in these lines (fig. 4).

Supposing that this cotton with Young's optometer is placed before this eye, all these lines will meet exactly

FIG. 4.



DIFFERENCE IN APPEARANCE WITH THE USE OF YOUNG'S OPTOMETER BETWEEN EYES WITHOUT AND WITH ABERRATION.

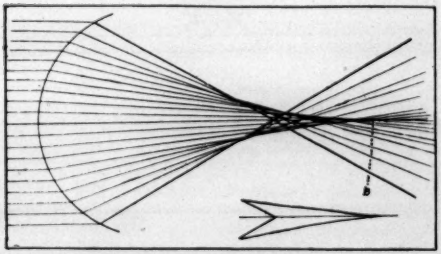
at a point; practically speaking there would be no aberration; but when we come to this one I want you to notice that the aberration is different on the two sides, and that it gives a sort of twisted appearance to it, which you can positively see by using Young's optometer —that is to say, that the lines on the one side come to a point long before those on the other side, showing, of course, that the one side is different from the other so far as aberration goes.

Now, you can quite see that, using Young's optometer, we can find the aberration of the far point, and

we can also investigate it for the near point. Of course, the difference between these two points represents the amplitude of accommodation. It is a very difficult measure to use, and requires considerable practice; but I can assure you it is worth doing. It is a thing you do not see at once, but it grows on you, and you can see the lines after a time much more clearly.

I have here two diagrams (figs. 5 and 6), which, probably, you may have seen before. They represent two corneas of eyes. Supposing one cornea is an arc of a sphere, and the other shows a cornea which departs from the arc of a sphere, and is over-corrected for spherical

FIG. 5.



POSITIVE ABERRATION—NOTE DIRECTION OF ARROW HEAD.

aberration. Now, of course, you understand that those rays from the centre and the outer portions do not meet in the same point.

They form a kind of arrow arrangement. I have sketched the arrow underneath, but I want you to notice that the arrow-head points in opposite directions, according as the eye is over-corrected or under-corrected for this spherical aberration, and it forms an easy means of remembering this fact.

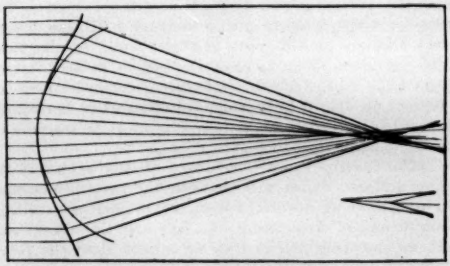
I have omitted Tscherning's experiments on dead eyes, in which he measured the surface, after submitting the lens to equatorial traction, because there are many objections to such a method. Hess has shown that the lens would not have the same form as when surrounded by

fluid, since in air it presses on its support with a weight of 174 mgs., while in water with only a weight of 11 mgs.

The observations of Heine and Hess have had much to do with the predominance at the present day of the older views upon the action of the ciliary muscle and the capsule. Heine has measured the radius of curvature of the crystalline *in situ* in the dead body, first with the zonule intact, and afterwards with it cut, and he finds a state not consistent with Tscherning's suppositions.

Hess seems to have been the first to investigate certain displacements of the crystalline during the act of accommodation, being induced to study the question further after noticing a peculiar trembling of the lens about the

FIG. 6.



NEGATIVE ABERRATION—NOTE DIRECTION OF ARROW HEAD.

period of maximum accommodation. He relied mainly upon a case where repeated iridectomies had removed nearly the whole of the iris, to investigate what appeared to be a downward movement of the body due to its weight when within the relaxed capsule. Further, he investigated another case where pigmented spots on the anterior capsule shared in the downward movement, thus indicating that this part of the capsule, at least, was relaxed.

Karl Grossmann has published the details of his researches upon a most interesting case of congenital absence of the iris, and in the main agrees with Hess as to the points which are in favour of the Helmholtzian

theory. He, however, found that the lens moved upwards and inwards after forced accommodation by the use of eserine. Unfortunately, his patient was highly hyperopic—to the extent of 6 D and 7 D in the two best meridians of the two eyes respectively, and only about 1 D or 2 D of accommodation was available for near vision. He has no doubt regarding the formation of an anterior and posterior lenticonus, and wherever his report concerns any of the details mentioned by Hocquard there is complete agreement.

Any theory of accommodation, to prove acceptable to the majority of modern physiologists, must take account of the findings—I believe in the first place by Young—upon the change in shape of the lens now so generally associated with the name of Tscherning. It is a remarkable thing that we in England did not discover the value of Young's work until a century had passed, and after foreign physiologists had awakened to the fact. It seems very difficult to reconcile them with the ideas of Helmholtz, and, indeed, Tscherning regards them as sufficient to disprove the older theory, and he maintains that only traction on the zonule could, for the purpose of accommodation, produce such changes.

Unfortunately I am not aware of any actual facts being adduced which will reconcile the careful anatomical researches of Hocquard with the equally painstaking observations of Tscherning—we feel that the one is correct, and we dare not, in fact, we cannot, deny the value of the other's investigations. Although no facts have been brought forward an attempt has been made to show that Tscherning's observations may not be inconsistent with the theory of Helmholtz, and that it is possible for such a curiously arranged structure as the crystalline to be so composed of elastic fibres as to assume conicity during relaxation of the equatorial tension, and sphericity with such tension.

Priestly Smith some time back exhibited in this city a model of elastic hoops so arranged as to show this peculiarity, but there is no actual proof, nor was any claimed, that the crystalline was similarly constructed.

It is certain that there is much still to learn with regard to the action of the structure of the crystalline upon light passing through it in its various states of accommodation. Shelford Bidwell made some rather re-

markable experiments with an electric lamp and a round aperture of 2 mm. diameter in a metal plate covered by a ground glass combined with one of ruby red (in order to obtain monochromatic light).

Using a concave lens of 3.5 D., at two feet distance, he saw seven bright spots, one central and six peripheral, upon a luminous ground, and increasing the distance between eye and plate the spots became elongated radially and divided, at the same time new ones being developed, and as the distance was increased, so the spots multiplied, until at 20 feet, the patch merely seemed to be mottled, but with some bright strings upon it which corresponded apparently with the sutures of the lens.

That is rather a curious point. Supposing this represents the disc of monochromatic light, here is a central spot, and there are six spots disposed round it fairly symmetrically. Now, each one of these divides almost like amœba, and ultimately the whole lot divide to such an extent that it becomes blurred, and then lines appear on the disc comparable to the sutures of the lens, and very much like what you will see in the entoptic phenomena by using the ordinary pin-hole and looking into the sky.

Unfortunately, Mr. Bidwell does not favour us with his amplitude of accommodation or the acuity of his vision, but it is tolerably certain that he could not entirely overcome the − 3.5 D, and as the distance varied from 2 to 20 feet we note that the differences in these phenomena were probably caused by what we may represent as merely the equivalent of the exercise of .5 D of the amplitude of accommodation.

He further experimented with convex lenses and slits, for the details of which I must refer you to his book "Curiosities of Light and Sight," but although the phenomena are similar he does not seem to take account of the fact that with concave lenses accommodation is certain to be exercised, and I believe further experiments upon these lines, added to a series made with Young's optometer, might add much to our knowledge. So far as I know no explanations have been given of this curious multiplication of the marks on the disc. Apparently we must conclude that it is due to some structure of the lens, but that they should multiply is puzzling.

The observations of Hess and Grossmann upon the movements of the crystalline lens during accommodation have appealed to me particularly, because of some notes of my own bearing upon the same subject. During considerable experience in supervising the centering of spherical and sphero-cylindrical lenses by others, I have been struck by the fact that when cross lines are used in a horizontal plane errors are very much more frequent than when the cross lines are in a vertical plane. Not only so, but the errors are almost always in the same direction with the same person, and to the same amount approximately, these points being most important.

To such an extent does this apply that I find for accuracy it is much more desirable to use cross lines in a vertical position and at a fair distance, marking the lens when still viewing the cross lines. The errors occur with weak lenses particularly, and you will at once see that the use of horizontal cross lines necessitates the exercise of accommodation, as they are always comparatively near to the eye.

This is undoubtedly correct. I know cases where the errors are invariably in the one direction, and nearly always of the same amount. Cross lines are used at a certain distance upon, we will say, a disc. I have never found that occur when the cross lines are vertical and removed at some distance. Of course you can quite see that in the case of using the cross lines horizontally there is accommodation exercised, and, apparently, there is some lenticonus, and, undoubtedly, if the lens is moved when in that position, it will cause the cross lines to be marked in the wrong place on the lens. You can readily try this by putting the prism in front of the lens when you are neutralising it, and you will see that you cannot mark the lens correctly.

Looking at the whole of the facts and arguments adduced we can scarcely feel justified in rejecting the theory of Helmholtz merely because of the formation of lenticonus during the act of accommodation. This we cannot understand at present, but to accept Tscherning's theory in its entirety would be to accept an explanation at variance with the careful observations of actual structure and function made by many of the most competent ophthalmologists of our time.

The Discussion.

The CHAIRMAN said he was sure that all would agree with him that they had had a most interesting and a most suggestive paper. There were several in the room who were quite competent to discuss the paper, and he would ask them to do so.

Dr. ETTLES said he thought the reader of the paper was to be congratulated on having put before them material for reflection, and it was interesting to hear that Helmholtz's views—or, rather, the views which had been attributed to Helmholtz—were still regarded as being sufficient to explain most of the phenomena of accommodation. He said those views which had been attributed to Helmholtz, because Helmholtz himself never formulated the theory of accommodation which was accorded to him. As Tscherning said in his book, his followers, more loyal than the King, proclaimed as accepted that

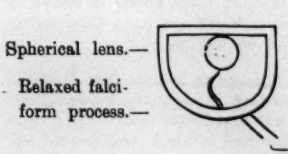


FIG. 1.

FISH EYE—RESTING STATE ADAPTED FOR NEAR VISION.

which he himself suggested. He thought, looking at accommodation in its broad view, one need not confine oneself to the accommodating human being; one might think of accommodation as exercised in the realms of nature generally. In the fish, for instance, they found a most wonderful adaptation to surroundings.

If he might have access to the blackboard—without which he was rather speechless—he would like to just draw a section of a fish's eye. Let them imagine a basin—an ordinary rigid basin—with a glass lid. That would represent the outline (fig. 1). They had a hemisphere closed in front by a flat cornea. As all of them knew who had taken the trouble to pick a haddock's head to bits on their breakfast table, it had a little white spherical lens which reminded one very strongly of a

sugar-coated pill, and babies had been known to swallow it in the hope of medicinal effects. The fish's lens was perfectly spherical, and in the living state almost a translucent sphere, lying immediately behind and in contact with the cornea.

They might have observed, if they were a little critical in regard to their breakfast haddock's eye, that there was attached somewhere to the surface a thread, known as the falciform process, from the Latin *falx* (a sickle)—

FIG. 2.

RESTING FISH EYE—PARALLEL RAYS FOCUS IN FRONT OF RETINA RAYS FROM NEAR POINT (*p*) HAVE CONJUGATE FOCUS ON RETINA.

because in life it was supposed to assume that direction more or less. In point of fact, it might assume any direction. The great point was that it was slack—not a tight string. Here, then, they had a slack transparent ap-

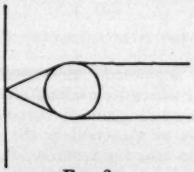


FIG. 2B.

ACCOMMODATING FISH LENS BROUGHT NEAR RETINA AND RAYS FOCUSING ON IT.

pendage, attached to the back of a transparent lens, so that the fish managed by its aid to decide whether the gudgeon in front of him was in good condition for his breakfast. It was curious that the fish's eye was always adapted for near work, and when the fish wanted to see at a distance it had to accommodate.

This spherical lens would have an extremely short focus—so short that it might almost be in contact with its posterior surface—but when immersed in water its relative index was so much reduced that it had a much longer focal length, so that the spherical lens of the fish, when immersed in water, would have a fairly long focus, and that focus would cross in front of the fish's retina (fig. 2 and fig. 2*b*). Now, that falciform process contained transparent muscular fibres, and when the fish wanted to see at a distance it tightened that string and pulled the lens nearer to its retina so that the cone came close to the retina, and it saw the distant object clearly (fig. 3).

This was, to his mind, a wonderful bit of accommodation which one met with in fishes, and which

FIG. 3.
FALCIFORM PROCESS TIGHTENED.
FISH EYE—ACCOMMODATION ADAPTED FOR DISTANT VISION.

was different from that met with in the mammalian eye. Now, with regard to the mammalian eye, the point about the disposition of zonule fibres was of interest in this way: that one was too apt to think of the fibres of zonula as being made up of laminæ, or plates. In point of fact they were simply fibres, like threads of cotton, passing from the ciliary process to the lens capsule somewhere or another. Of course (said Dr. Ettles) he knew of Hocquard's investigations—still, there were many others who had been at work on that.

If they examined the section they found that the processes of the fibres passed from the posterior portion of the ciliary process to the anterior portion of the lens capsule cut across in the shape of an X—they did not really come forwards as represented in books (fig. 4).

Not only that, but if they made a section transversely to the zonule, the fibres would be seen cut across as isolated dots (fig. 5). There were no plates and no lamination.

Now, leaving the facts of accommodation, let them see how far it was possible to explain them according to the other view. One of the most dominant points was that they had lenticonus. If they took a sheep's eye and pulled out the crystalline lens and broke it up be-

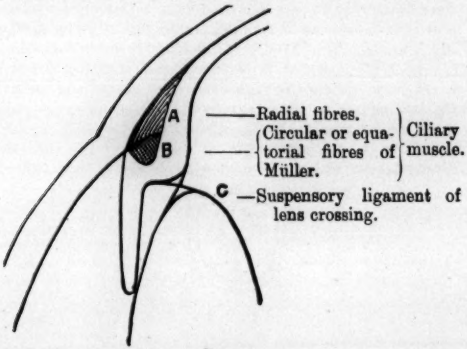


FIG. 4.

tween the finger and thumb they would find an outside cortex, bark, or covering enclosing a hard nucleus. That nucleus was quite resistant to the fingers. One could separate it, wash it in water, and take it in the

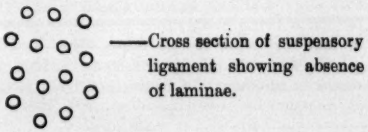


FIG. 5.

hand a stiff, hard lens very much like a fish's lens. That was to say, they had a strongly resistant gluey-like lens nucleus surrounded by a gelatinous cortex (fig. 6).

Now, in Helmholtz's theory so called, there was no part allotted to that nucleus, but in point of fact, it must have a very strong bearing, and Tscherning made it the basis of his theory, he said, "Here we have a

nucleus, and when we alter the shape of the lens it assumes the lenticonus position by being moulded, or pressed, on this hard solid spherical central nucleus." (fig. 7). It was imagined at one time that the bundle

Structure of Lens.

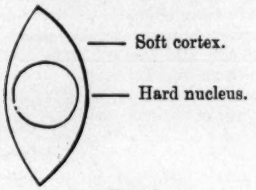


Fig. 6.

of muscle being wound round could press upon the lens margin and actually mould it.

Later on, by certain methods, it was found that those circular fibres left their concentric arrangement, and

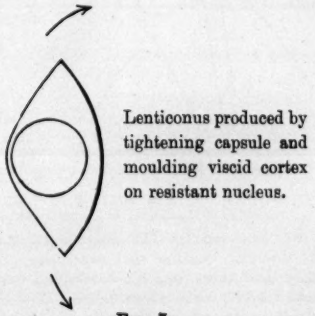


Fig. 7.

spread themselves out in a radial manner. When that muscle contracted, that portion to which it was firmly attached was bound to remain stationary. They did know that the choroid passed in that direction, there was no doubt about that.

For one thing the experiments of Hensen and Völkers proved it. They exposed the eye of living animals and passed through the sclerotic fine needles, whose points were embedded in the choroid, and then by stimulating the ciliary nerves they caused the contraction of the ciliary muscle and the iris. The result was, that the needles, which were embedded in that manner when under stimulation, became oblique, showing that the choroid undoubtedly slid forward under the contraction of the ciliary muscle (fig. 8).

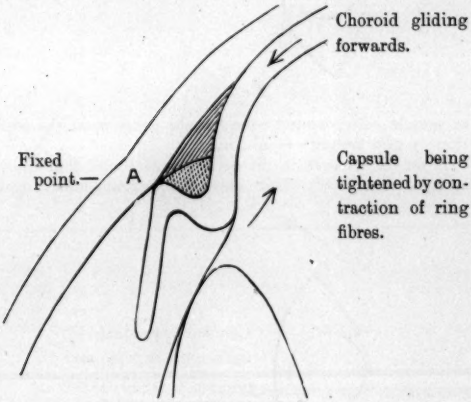


FIG. 8.

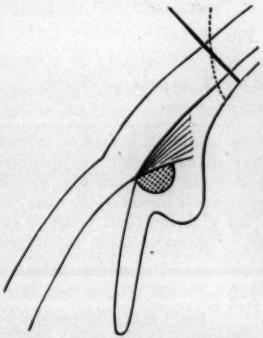
That was one point. The next point was that the anterior chamber became shallower, the pressure was diminished, and there was the celebrated experiment of Förster who had a case where there was a little tunnel or fistula into the anterior chamber; he put a drop of fluoresceine in and told the patient to accommodate, and during the act of accommodation the stain entered the anterior chamber.

They knew that there was lenticonus, by adopting Young's optometer, which was a very good way of showing it, or by using Scheiner's test, which was very much

the same—a disc having two holes less far apart than the diameter of the pupil.

Provided that the pupil was more than 5 mm. in diameter, what did they find? That by means of those two holes it was quite easy to find out the refractive conditions of the eye. Without making too long a story about it, one found that 2½ mm. from the centre of the lens one only got half the central refraction. Another one was to take three little electric lamps—the idea is Tscherning's—with a telescope below. That conclusively proved that the anterior surface of the lens became much more convex in its centre than in its periphery.

On looking at the eye through the telescope one found three images in a row, reflected from the anterior surface of the lens. On accommodation being exercised, the



Hensen & Völckers' experiment. Needles passed through Sclerotic and choroid. Black line = position at rest—dotted line = position on stimulating ciliary nerves showing gliding of choroid.

FIG. 8 (*a*).

central one dropped towards the centre. That is, the radius of its reflecting area became shorter than the peripheral radius, so that the surface was one of the second degree.

He must advert to the only strong argument which had yet been brought forward to attack Tscherning's view; the only one which could be regarded as serious, and which would give pause to the exponents of his view, that of the lens having been found to drop down a little

when accommodation took place. Recently he had had a very good opportunity of studying that in a case of a person with posterior polar cataract.

If a pin-hole disc were placed at the anterior focus of the eye, rays passing through the optic media would be parallel, and if there was any opaque object in the path

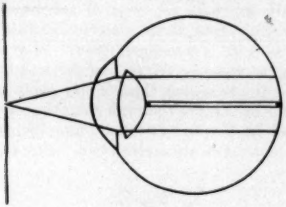


FIG. 9.

POSTERIOR POLAR CATARACT SEEN ENTOPTICALLY BY PIN-HOLE PLACED AT ANTERIOR FOCUS, SHADOW OF OPACITY THROWN ON RETINA.

of those parallel rays there would be a shadow formed upon the retina, and that shadow would be projected upon the ground glass and could be drawn (fig. 9). It came to this, that the person under observation, when the little hole was placed at the anterior focal point of the eye, could see the opacity. Instead of using the instru-

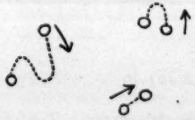


FIG. 10.

ment on a table like a microscope, it was attached rigidly to his head.

The base of the instrument was a bit of semi-soft wax, and then from that upwards there was this little disc placed at the anterior focus of the eye, so that wherever the head moved it was in the anterior focal plane. What he found was that it did not consistently

drop, but underwent a series of deviations not yet to be clearly explained (fig. 10).

Mr. DIXEY said he would like to give some expression of thanks to the two gentlemen who had spoken for their instruction on the subject of accommodation. There was very little one had to say in comment, or, rather, that he personally had to say. It was a subject of natural interest to everybody who had anything to do with the eye, but the minute anatomical examination of the subject was not what they had all, perhaps, been able to indulge in. There were two questions of great interest connected with the accommodation of the human eye. He would not say that he was disappointed, because they were a little outside the subject as the lecturer had taken it, but he would have been extremely pleased if Mr. Taylor had mentioned them in the course of his interesting paper.

One of them was the possibility of irregular, or sectional, accommodation, as it was called, and the other was the possibility of unequal accommodation. It had occurred to him, with those two problems in his mind while listening to the paper, that possibly the displacement of the lens to which Dr. Ettles had alluded might be used to explain the involuntary correction of a corneal astigmatism. He did not know whether the possibility of that had ever been considered. It had also occurred to him that Young's optometer might be used to demonstrate cases in which sectional accommodation was supposed to exist, and he would like to know whether the method employed by the optometer was capable of being adapted with sufficient exactness to determine the existence, or otherwise, of the irregularity. There was one other little observation that had interested him, with which Mr. Taylor concluded his paper.

He spoke of the examination of cross lines, and compared the effects of examining them in a vertical and a horizontal position; he confessed it was a little difficult for him to accept Mr. Taylor's observations in that respect. He should have thought it was a simple question of the position of the eye relative to the cross lines of the lens that was being tested. As long as the three points were in alignment he did not see how there was room, short of a miracle, for anything happening other than what one could expect. The question of difficulty

arising when the cross lines were used inside the area of accommodation was not difficult to meet practically, because they might take as one point of alignment a pin-hole disc, which made accommodation practically unnecessary.

Mr. TAYLOR said that, of course, one required a very great amount of confidence to differ in view from Dr. Ettles even in the smallest particular. There was one point, he thought, in which he did not quite agree. He was not quite sure that the various positions which the capsular mark took were quite proved, because, supposing that the lens was relaxed, he believed it was quite possible, in fact, observations had been made, that the lens did not always fall in the same position, and that the lens might twist when that spot would not move in just the same way as if the lens fell. With regard to that movement, he had not mentioned it in his paper, but he had noticed many times, in some experiments which he had made himself, a very curious tremor at the maximum of accommodation, especially with Young's optometer. When they got towards the near point, and accommodation seemed to become strained, there was a decided quiver—of course, he knew Tscherning would say it was a tightening up—he believed Tscherning had explained all those things—but it had always seemed to him a great difficulty to explain it in that way.

Dr. ETTLES: Have you seen it quiver?

Mr. TAYLOR: Only in one position—the near point.

Dr. ETTLES: A subjective quiver?

Mr. TAYLOR: Yes.

Resuming, Mr. Taylor said, with regard to Mr. Dixey's remarks, of course, if they had a pin-hole and the apparatus complete, the error could probably be got over. The remarkable thing was that the error could be obviated by the very persons who made it. It seemed as if they could make the correction, but he might tell them that in one particular case he was prepared to say that his observations extended to a thousand instances, and that the amounts were fairly constant. Moreover, he knew of several cases where the error was different in direction—that was what puzzled him to some extent. Why not always in the same direction? He merely recorded the case; he could give no adequate explanation, except that they had the observations of Hess and Gross-

mann totally different, the one maintaining that the lens fell and the other that the lens rose.

The CHAIRMAN said that he was quite sure that whatever views one might take of the rival theories of accommodation which had been put before them, there was one point upon which they were all agreed—or more than one. The first was that Mr. Taylor had given to this subject an amount of minute attention which they could all admire; and, again, that he had placed before them clearly and lucidly the various points for and against the different theories, and by so doing had contributed very much to their enjoyment. He felt that the subject had been thrashed out so thoroughly by Mr. Taylor and by Dr. Ettles, that he himself would not venture to make any criticism at all. He could assure Mr. Taylor that he personally had enjoyed the paper, and he hoped they would allow him to convey to him a very cordial vote of thanks, with the additional assurance that that delight had extended to every member of the Society in the room.

ENTOPTIC VISION.

PAPER BY PROFESSOR W. F. BARRETT, J.P., F.R.S., ETC.

Read before the Optical Society on June 28th, 1906.

THE sense of sight is intended and adapted for the perception of objects in the external world, and, accordingly, that wonderful instrument, our eye, has been trained to perceive and to localise objects in the world outside ourselves; but the retina is constantly receiving impressions from within the eye as well as from without. Those impressions are usually disregarded, for, as a rule, the impressions derived from objects within the eye are neither novel nor insistent, and we pass them by. Under certain conditions, however, they are rendered more pronounced, and we are able to perceive them.

Now, the perception of phenomena, the seat of which is within the eye, has been termed *entoptic vision*, whilst the perception of phenomena, the seat of which is outside the eye, that is to say, our usual and ordinary vision, we may call *ex-optic* vision. To-night I am concerned only with the former—entoptic vision.

Everybody has, no doubt, noticed, and many people have been needlessly alarmed at the perception of, moving shadows which flit across the retina, resembling pearly strings or notes of interrogation. These are called *muscæ volitantes*, or flitting flies. They are due to the shadows thrown upon the retina by the débris of cells which are floating in the vitreous humour of the eye. They can be perceived with a general illumination of the eye when looking at a bright surface; even when the

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This paper contains extracts from and is a summary of papers on Entoptic Vision, read before the Royal Dublin Society, and published in their proceedings, Vol. XI., Nos. 7 and 8, 1906. The illustrations reproduced by permission of the Royal Dublin Society.

lids are closed in bright light they can be seen; the reason why they are seen under general illumination is because they are close to the retina. If, however, instead of a general illumination of the eye a point of light be used near the eye then the shadows become much sharper and clearer. And under such circumstances these muscæ volitantes can be examined and drawn, and studied with the greatest ease.

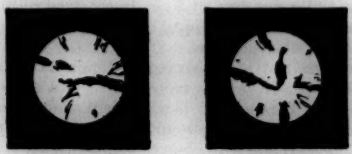
There are various ways of obtaining a luminous point. We may use, for example, the convex surface of a silver teaspoon, or, still better, a silver bead. We may even use the rim of a pair of spectacles. At the present moment the rim of my spectacles acts as a convex surface, and is reflecting into my eye a point of light from the electric light beyond, and now that I know what to look for, I see the pupil, the obscurities in my lens, and certain other minute obscurities within my eye, all before me, as I am speaking to you. I have devised a simpler arrangement to replace the spectacle-rim. A spectacle frame is taken, and fixed across it is a wire, attached to the centre of the wire is the smallest sized bicycle ball; thus a very intense spot of light is obtained quite close to the eye, and these entoptic phenomena can then be seen with singular clearness.

Another method of obtaining a luminous point, or *homocentric* pencil of rays, is by the use of a short focus convex lens. This method is convenient under some circumstances. In high-power microscopy entoptic phenomena often interfere with observation, as here also a brilliant focus is formed near the eye.

A simpler method of getting a luminous point is by means of a pinhole in a piece of metal or card. If such a piece of pierced cardboard is held to the eye and a bright light looked at, all these entoptic phenomena will be seen. The first thing perceived is a disc of light, and most people suppose that this is the image of the pinhole. It is, however, the image of the pupil of the eye —the shadow of the iris thrown upon the retina. And, thus, by this absurdly simple means—a piece of card pierced with a pin-hole—any slight obscurity in the eye can be seen, the diameter of the pupil can be determined, and a number of interesting phenomena connected with the contraction of the pupil can be studied with ease.

I happened to be observing, about a couple of years

ago, a point of light thrown upon the screen at one of my lectures, and noticed that the light was crossed with a faint line, and looking through a pin-hole at the screen I saw this still more clearly. It was not present in the left eye, but in the right. I examined my right eye by a pin-hole screen from time to time, but could not notice much alteration in the shadow. After a time the shadow appeared in the left eye. This went on for about six or seven months, when I consulted an oculist, but he could not see any obscurity in the eye. However, it increased and I again consulted him, when he discovered there was a cataract in the right eye, and a trace of cataract in the left. I had been observing that for several months. Then I set to work to improve my arrangement for observing these phenomena, and gradually brought into shape an instrument (described in the sequel fig. 10) by means of which I was able to draw the projected shadow caused by the growth of the cataract. A rough drawing I made of what I saw in my right and left eye is shown in fig. 1.



LEFT. RIGHT.

FIG. 1.

The discovery of this entoptic mode of examination of the eye was made by Professor Listing, of Germany, some 60 years ago. It is to him we owe the term "Entoptic Observation." The subject was taken up almost simultaneously in England by Sir David Brewster. Others who studied the subject were Donders, Doncan, and Dr. Jago. Snellen and Landolt, in their great work, have a chapter devoted to entoptoscopy. Of late, however, the subject has been overlooked and practically lost sight of. Donders, in his work on the "Accommodation of the Eye," says: "Now that the ophthalmoscope is come into use the entoptic mode of

examination for diagnosis will be thrown into the shade." But with all deference to Donders, I doubt this. It is a remarkable fact that neither he nor any of those who formerly worked at this subject had tried it with a person who had a cataract. Drawings are given of what they and their pupils saw, but only of minor obscurities and of the structure of the lens.

In looking through various English text books on ophthalmology, I find there is no reference in any of them to the value of entoptic observation, and it seems to have fallen out of use in general practice, and no doubt for a very good reason, because subjective examination of the eye is not, generally speaking, so trustworthy as objective. Nevertheless, for certain purposes the former may become exceedingly important.

Now, we may ask at the outset, how is it that we can perceive small obscurities on the lens, or within the eye when an obscurity on the lens of an ordinary photographic camera is not seen in the image it forms? If you have the camera focused on a given object and use a diaphragm in front of it you only perceive the image of the external object and not of the diaphragm. If you place your finger before the lens you do not perceive the finger. How is it, then, that we can perceive things in our own crystalline lens that we cannot perceive in the lens of an ordinary camera? The answer is, that you have the external image sharply focused upon the camera screen; but throw the image out of focus and then you will see blurred shadows of objects near the lens. Both theory and experiment teach us that this must be so.

Now, if, instead of using a general illumination of the camera we use a point of light obtained either from an electric lamp or by means of a card pierced with a pin-hole, and keep the focusing screen out of focus, we see a clear and sharp outline of any small object on the lens or anywhere in the path of the rays. It is exactly the same in our eye, for our eye is a camera obscura. When the illuminated pin-hole is placed at such a distance from the eye as to be within the power of accommodation of the eye, usually a distance of about 9 inches or a foot, a sharp image of the pin-hole is focused on the retina, and no shadow of any small object in or near the crystalline

lens can be perceived. When, however, the pin-hole is brought close to the eye, accommodation fails to focus its image on the retina, and the shadow of any object in the path of the rays will be clearly seen, and the more sharply the smaller the pin-hole; whilst the circle of light seen on the retina is now not the magnified image of the pin-hole, but the shadow of the iris diaphragm of the eye, as has been already stated.

Entoptic observation was evidently not known in Shakespeare's day. In *Julius Cæsar*, Act i., sc. ii., line 51, Cassius says:—

> "Tell me, good Brutus, can you see your face?
> *Brutus.* No, Cassius; for the eye sees not itself,
> But by reflection by [from] some other things.
> *Cassius.* 'Tis just;
> And it is very much lamented, Brutus,
> That you have no such mirrors as will turn
> Your hidden worthiness into your eye,
> That you might see your shadow."

To those who, like myself, suffer from presbyopia, and have lost the power of accommodation for near objects, entoptic observation can be carried on with a large pin-hole, say, 1 mm. in diameter, or any bright spot of light placed at a considerable distance from the naked eye; in my case up to 20 inches. Though the entoptic shadows are now smaller, they are darker and less detailed, and hence their outline can be more easily drawn under such circumstances. To exclude extraneous light a sliding brass tube, blackened inside, can be made to screw over the pin-hole aperture, so that the observer can adjust the length of this tube, through which he views the aperture.

Of course if the presbyopic observer puts on his spectacles the distant pin-hole is now focused on the retina and entoptic observation ceases until the aperture is brought so close to the eye that even with spectacles its image cannot be formed on the retina. *Ceteris paribus* the same thing holds true with myopic vision; hence, spectacles of any kind are of no use in entoptic observation, and may entirely destroy the effect sought for.

As the anterior principal focus of the eye may be taken as 14 mm. distant from the cornea, when the pin-hole is placed at this distance, the divergent cone of rays enter-

ing the eye will be rendered parallel, and hence the shadow of any object in or between the crystalline lens and the retina will be practically of the same size as the object itself. Obviously, when the illuminated pin-hole is placed nearer to, or further from, the eye than the anterior focus, the shadows will be respectively larger or smaller than the object.

This is shown in the accompanying diagrams, where fig. 2 represents the luminous point *a* (or stenopaic screen *s*, *s*), placed at the anterior focus, the enlarged entoptic object *b* throwing the shadow β; the rays being parallel within the eyeball, the area of the shadow is the same as that of the object.

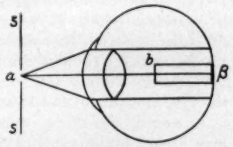


FIG. 2.

There are two interesting facts in connection with entoptic observation which require a brief explanation. On looking through a minute aperture in a card held close to the eye, it will be noticed that an object, such as a pencil-point, can be seen with perfect distinctness, even when brought within an inch or two of the eye, *i.e.*, far within the limit of clear vision; and this notwithstanding that the observer may be hypermetropic or presbyopic. The fact is well known, and is sometimes used for enabling very small print to be read with ease. The accompanying diagram, fig. 3, will help to explain this anomaly. If a small object, *a*, *b*, be placed near the eye, the screen with pin-hole aperture *s*, *s*, in the first instance being removed, the pencil of rays from the object reaching the eye at m^1, m^2, would be refracted and encounter the retina at *g*, *h*, and *i*, *f*; the prolongation of these rays would unite at β and ^{a}, when a clear inverted image of the object would be formed and perceived, if the retina were at that distance. As it is, however, a blurred and wholly indistinct image is formed on the actual retina, the cross-section of the cone of rays from

the points *a* and *b* being shown at *g*, *h*, and *i*, *f*. These are *the circles of diffusion* corresponding to those points, and so with every other point of the object. If, now, a stenopaic screen, *SS*, be interposed, the luminous cone which enters the eye is restricted to the pencil of rays from the minute area of the pin-hole; and, consequently, all the sections of the cone up to its apex, *i.e.*, the area of the circles of diffusion, are proportionately diminished, so that where the retina intersects the cone of rays, this section has become practically a point; the circles of diffusion (which are areas of confusion of the image) have vanished; and a sharp image of *a* is formed at *f*, and of *b* at *g*. Moreover, *the magnitude of the retinal image is greater than before*, coinciding, as will be seen, with the exterior boundary of the rays.

If the screen *SS* were removed lines drawn from *g* and *f*, passing through the nodal point *k* of the eye, determine the direction in which the retinal image would be seen projected from the eye, and an inverted magnified image of the object *a*, *b*, would, therefore, appear at *G*, *H*. Owing to the large visual angle subtended by an object held near the eye, a microscopic object, such as a transparent scale, divided into fifths of millimeters, can be clearly seen when held close to the eye, and viewed through a very minute aperture in an opaque screen. The amount of light reaching the eye through a minute aperture is, of course, very small; but if the light be sufficiently good this simple method affords an excellent way of seeing minute objects, or of reading well-illuminated small print, when a lens is not at hand, or spectacles are inadequate.

If, instead of holding a small object on the far side of the pin-hole screen *SS*, it be held between the screen and the eye, a *shadow* of the object, and not an inverted, lenticular image, will be thrown upon the retina. This shadow will be erect, corresponding to the position of the object, but will be projected from the retina as an inverted image; thus, if a pin, with the head upwards, be held between the screen and the eye, an enlarged image of the pin with the head downwards, will be seen. In like manner any obscurity within the eyeball, when revealed by pin-hole vision, is seen completely inverted laterally as well as vertically, though, as with the pin, the shadow of the obscurity on the retina is not inverted.

The fact that in *ordinary* vision we see things erect, though the retinal image is inverted, presents no real difficulty. The mind does not contemplate the retinal image as a photographer views the inverted image of a landscape when focusing his camera. In fact, we are wholly unconscious of the existence of the retina, or of the size or position of the retinal image, except from experiments on eyes other than our own. Visual perceptions are always referred to a position in space external to our eyes, and are projected outwardly by our mind along the "line of visible direction," as Brewster termed it. Now, this line is almost exactly coincident with a straight line joining any point in a visible object external to the eye, *if seen by direct vision*, with the corresponding image of that point on the retina. These lines necessarily cross each other at *k*, fig. 3, which is the *centre of visible direction* in the eye. The inverted image, therefore, gives rise to the perception of an erect object and *vice versa*.

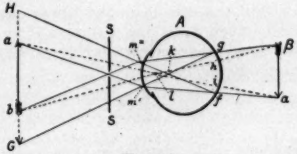


FIG. 3

The centre of visible direction *k*, fig. 3, may be taken as coinciding with the optical centre of the eye. The optical centre of a thin lens is such that all rays not remote from the axis passing through it are undeviated by the lens; but every lens, or system of lenses, of which the two faces are not in the same medium, as is the case with the eye, has two geometrical points which replace the optical centre of an infinitely thin lens. These two points are termed *nodal points;* and these in the eye lie so close together that they do not sensibly differ from a single point. This is situated on the optic axis close to the posterior (the inner) face of the crystalline lens, about 7 mm. from the surface of the cornea, and 16 mm. from the retina. At this point, the object *O* and its retinal image *i* subtend the same angle, and the relative

magnitudes of object and image are in the direct ratio of their relative distances D and d from this point, or,

$$\frac{i}{O} = \frac{d}{D}.$$

Hence, as $i = O\frac{d}{D}$, the retinal image formed of a meter-stick held 1.6 meter from the nodal point (*i.e.*, 1593 mm. from the cornea) will be exactly 1 cm. long.

What determines the lines of visible direction in the case of entoptic phenomena opens up some interesting questions in relation to vision, into which I cannot now enter.

Assuming that the perception of entoptic objects follows the same law of projection as that of external objects —and experiments indicate that this is the case—an obscurity 1 mm. in diameter near the retina will be perceived as an inverted image 1 cm. in diameter at a distance of 16 cm. from the nodal point, or 15.3 cm. from the cornea. Hence, the linear magnitude x, of an obscurity within the eye, is to the linear magnitude of the projected image S, as the distances d and D of the nodal point from the retina and from the projected image respectively, or

$$x = \frac{d}{D}S.$$

It is, therefore, quite easy for anyone to determine for himself, without using instrumental appliances, the magnitude of any small obscurities within the eye. For example, the *muscæ volitantes* which float near the retina, throw shadows that are perceptible without using homocentric light. They can, if present, readily be seen upon looking at the sky or any illuminated surface, such as a sheet of white paper placed at a convenient distance from the eye. Keeping the head and eyeball as far as possible motionless, the observer can mark with a pencil on the sheet of paper, the length and breadth of the projected image of one of these muscæ. For this purpose the paper can be about one or two feet (30 to 60 cm.) from the eye, so as to be within easy reach of a pencil held by the observer. If the image is too small at this distance, the paper can be placed further off, and the size of the projected image judged by marks previously

made on the paper. Knowing the distance of the paper, the length and breadth of the retinal shadow is to that of the projected image as the distance of the nodal point of the eye from the retina (viz., 16 mm.) to the distance of the sheet of paper from the nodal point, or practically from the eye. Thus, in the case of a particularly persistent and large musca I recently noticed in my right eye, the length of the projected image of the filament was about 150 mm., and its breadth 2 mm., the sheet of paper being 60 mm. from the eye. Accordingly the retinal shadow was —

$$\frac{16}{600}\,150 = 4 \text{ mm. long,}$$

and

$$\frac{16}{600}\,2 = 0.053 \text{ mm. broad.}$$

As this musca was very near the retina its actual size was practically the same as the above.

In making this experiment one eye must be shut, and the eye used should be partly closed so as to give a sharper and darker shadow. Looking through an aperture made in a piece of card with a stout needle, gives a much better result, as already explained. In this case the head can be rested on a support—such as the left hand, with elbow on the table—the eye being a foot or so from the sheet of paper, and the perforated card held or fixed closed to the eye; excellent drawings and measurements of the reticulated and cellular structure of the muscæ, with the knots and loops of their filaments, can thus be made. Their mobility is at first very provoking, but they are much steadier when the head is bent over the illuminated sheet of paper lying on a table, than when the head is erect and the paper vertical.

We owe to Sir David Brewster the first determination of the magnitude of these muscæ and the first suggestion as to finding their distance from the retina. He used two lights placed at a distance from the eye, a double shadow of a particular musca was thus thrown on the retina; the lights were then moved closer together till the projected image was touched. Measuring the distances of the lights apart and their distance from the eye, and the angle subtended by one of the images of the musca, enabled him to deduce its size and distance from the retina. The nodal point of the eye was taken by Brew-

ster as 0.524 of an inch from the retina, a little less than its true value, the width of the musca he found to be 0.0012 (1/820th) of an inch, this corresponds to 0.03 mm.; and its distance from the retina 1/85th of an inch, equal to 0.3 mm.; but their distance varies, and is often further from the retina than this.

The distance of entoptic objects from the retina, as Listing has shown, can be ascertained by noting the relative parallactic displacement of the shadow which occurs upon moving the eye.† This will be readily understood from an inspection of fig. 4, which is accurately drawn to scale. Within the eyeball let us suppose that three entoptic objects exist: No. 1 between the pupil and cornea; No. 2 in the crystalline near the plane of the

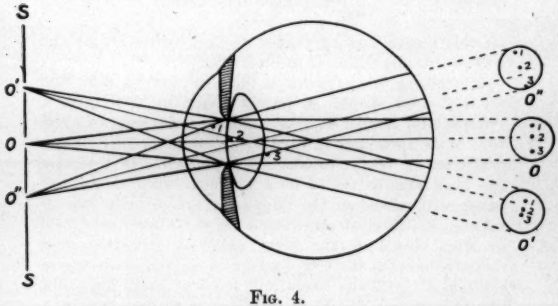


Fig. 4.

pupil; No. 3 in the vitreous; therefore nearer the retina than No. 2. The eye is first directed to the central spot of light or opening *O* in a stenopaic screen. The shadows of the three entoptic objects fall on the retina, and are seen as in the central projection. If now the eye be turned to view first the upper light or opening *O'*, and then the lower opening *O''*, the shadows are seen on the retina displaced as shown in the upper and lower projections. The entoptic object No. 2 in the plane of the pupil has not changed its position, but No. 1 in front of the pupil, and No. 3 behind, have both been displaced,

† Listing, 'Beitrag sur physiologischen Optik.' Göttingen, 1845.

but *in opposite directions*. Obviously this displacement or parallax is greater as the distance from the pupillary plane is greater, and thus the position of an obscurity within the eye can be found by the observer with a certain degree of accuracy.

This method, however, is not applicable to moving objects like the *muscæ*, and in any case requires a considerable amount of skill on the part of the observer. Brewster's method is better, and was improved upon by Donders by using two adjacent openings in a stenopaic screen: each aperture about 0.1 mm. in diameter and about 2 mm. apart.* Two overlapping images of the pupil are now seen as shown in *a b* (fig. 5). The closer the pin-holes are together, the more the circles overlap; the distance apart of the centres of the two circles exactly corresponding to that portion of the double image

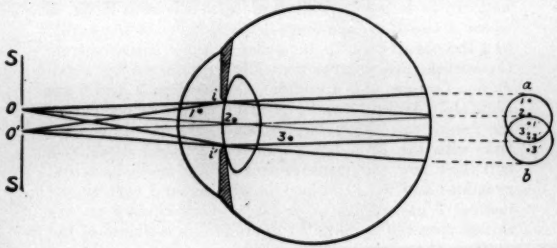


FIG. 5.

which does *not* overlap: and this distance, when the entoptic rays are parallel, is proportional to the distance between the pupil and the retina. If now an entoptic object, such as No. 2 (fig. 5), lie in the plane of the pupil, its double shadow will be as far apart as the images of the pupil, *i.e.*, as the centres of the two circles. On the other hand, the doubled shadow, if the object lie in front of the pupil, as No. 1, will be further apart

* 'Nederlandsch Lancet,' 2nd Series, D. 11, 1847. 'Archiv. f. physiologische Heilkunde,' viii., 1849. 'Accommodation and Refraction of the Eye,' by F. C. Donders, 1864, p. 203.

than the centres of the two circles, and if between the pupil and the retina, as No. 3, the shadows will be closer together. It is now only necessary to find the breadth of the part of the circles which do not overlap in order to find the relative position of an obscurity in the eyeball. For the distance apart S of the duplicate shadow is in the same ratio to the distance R of the entoptic object from the retina, as the distance apart C of the centres of the two circles is to the distance P of the pupillary plane from the retina; or

$$\frac{S}{R} = \frac{C}{P} \text{ hence } R = \frac{S}{C} \text{ P.}$$

Now, the distance, P, of the pupil from the retina in a normal eye, may be taken as 19 mm., in a myopic eye about 21 mm., and in a hypermetropic eye about 17 mm. Hence, the exact position of the entoptic object in the eyeball can be located. This is more clearly seen in fig. 6, where A and B are the over-lapping images of the pupil; and the obscurities, in the order of their distance from the cornea, are diagrammatically represented by a triangle, a square and a circle. As both figs. 5 and 6 are accurately drawn to scale, by using a pair of compasses, the reader can verify the law above stated. In connection with the Entoptiscope to be described directly, I will show how this measurement can be made in actual practice; and will also describe another and very simple method I have recently devised, which enables an unskilled observer to make an approximate estimate of the position of the obscurities.

By making a large artificial eye, and using a brilliant point of light, it is possible to illustrate in a lecture-room the whole of the principles of entoptic observation and measurement. This was shown at the conclusion of the paper, a small arrow painted on a strip of thin glass or mica being used to represent the obscurity within the eye and placed in different positions in the imaginary eyeball; the shadow of this object being thrown on a ground-glass screen to represent the retina. The source of light was an electric lamp the rays from which passed through an orifice (about a mm. in diameter) in an opaque screen placed before the artificial eye. When the ground-glass screen was moved out of focus, the sharp shadow of the arrow was seen wherever it was placed in the eyeball.

The effect of varying the size of the orifice in the opaque screen upon the visibility of the shadow of a small object, held at different parts of the eyeball, can thus be strikingly demonstrated to an audience.

With two closely adjacent pin-holes two partly overlapping circles of light were thrown on the screen; the distance apart of the double shadow of the object thus caused was seen to be exactly the same as the distance apart of the centres of the two circles, when the object was in the plane of the pupil; but this distance diminished in proportion as the object approached the retina. In fact the exact distance of the object from the artificial retina could thus be readily determined in a lecture experiment.

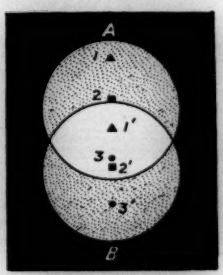


FIG. 6.

I will now briefly describe the instrument which I have devised for viewing, delineating, and measuring entoptic objects, and which I have termed the Entoptiscope, though its practical use must be left in the hands of the ophthalmic-surgeon.

In this instrument the observer *looks through* the pin-hole orifice on to a brightly illuminated ground-glass screen, placed upon what corresponds to the stage of a microscope. By this means the magnified image of the entoptic object is projected upon the ground-glass below, and the shadow can easily be traced with a pencil. An important feature of this arrangement is that the pencil-point appears to be exactly in the plane of the

projected shadow, so that no difficulty is experienced in making a tracing. The observer, if hypermetropic, will be astonished to find, on looking through the minute orifice, how distinctly he sees the sharp point of the pencil, or of any small object in the field of view, albeit far within the limit of clear vision of his unaided eye. This is due to the homocentric pencil of rays through the pin-hole orifice, and has already been explained.

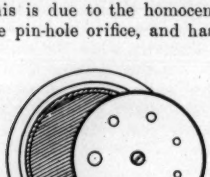


FIG. 7.

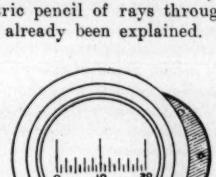


FIG. 8.

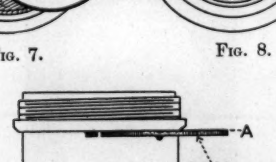


FIG. 9.

The first form of my entoptiscope consisted of a pair of vertical brass pillars supporting a head-rest which can slide from side to side, so as to bring either eye vertically over the pin-hole contained in the revolving diaphragm of the eyepiece. This diaphragm has pin-hole apertures varying in diameter from 0.1 to 0.5 of a millimeter, and a pair of pin-holes each 0.1 mm. diameter, and 2 mm. apart, so that by revolving the diaphragm either a single aperture of any given size or a double aperture can be successively brought before the eye. Figs. 7 and 9 show the diaphragm *A* in plan and elevation (the orifices are shown enlarged); the object of the double

aperture will be seen later on; an eye-cup screws on to the upper part of the eyepiece (fig. 9).

Below the pin-hole eye-piece is a transparent scale (fig. 8), divided into fractions of a millimeter; the shadow of this scale much magnified is projected upon the ground-glass stage below, along with the shadows of any opacities seen in the eye. At the base of the instrument is a mirror, which can be adjusted so as brilliantly to illuminate the eye-piece, using the light of the sky or that of a lamp. A sharply-pointed and hard pencil is used by the observer to trace the image seen on the ground-glass stage; and, as stated, pencil-point, scale, the image of the pupillary disc, and the projected shadow of the opacities in the eye are all seen in the same plane with perfect clearness. Hence, if the head and eye are kept steady, the drawing or tracing is made with ease even by an unskilled observer. The only difficulty consists in the contraction and then apparent dilatation of the pupil; the former occurs even in uniform light during accommodation, when the attention is concentrated in the act of drawing. This difficulty, however, is overcome by keeping the eye fixed for a few moments on the pencil-point before the tracing is made, or by dilating the pupil with homatropine when a careful drawing is required. The latter, a phantasmal dilatation, I will return to presently. After the drawing has been made the ground-glass can be removed and a photographic print taken, which is kept for comparison with the tracing made by the observer at a subsequent time. In my own case, so minute and clear are the shadows of the thickened tissues of the crystalline lens, that I can detect the slightest change, noting the gradual extension of the opacity, and could watch week by week the effect of any prophylactic if such were known.

For convenience of observation, in a later form of the apparatus, shown in fig. 10, I have hinged the vertical pillar *P*, so that the observer, when using the instrument, may incline it to suit himself; a single pillar is used in order to leave the hand free to draw on the ground-glass stage *G*, which carries a supporting hand-rest *R*. The eye-pieces *EE* have shaped cups to fit the eye and bring the cornea within a definite distance of the pin-hole. In this way the pin-hole can be placed at the anterior focus of the eye (about half an inch from

the cornea), for the reason already explained; and the stage is placed at a fixed distance so as to give a definite magnification.†

It is important in using the Entoptiscope that the observer should be comfortably seated and completely at ease; he should have his hands free and not be troubled

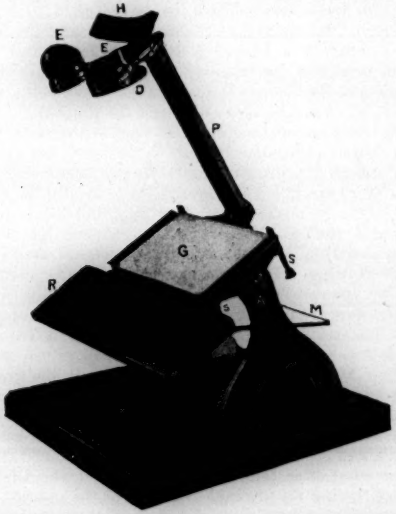


FIG. 10.

to keep one eye closed; it is much better, in fact, to keep both eyes open. The eye that is not under observation is kept in complete darkness by turning *D* until the index marks *O;* at this position there is no aperture in the diaphragm; thus either eye can be occluded with ease. The apertures are numbered in order of their

† The Entoptiscope, which is patented, can be obtained from Messrs. Baker and Company, High Holborn, London.

size, No. 1 being less than a tenth of a millimeter in diameter, No. 2 larger, No. 3 larger still, and so on. The mirror *M* is made sufficiently large to cover the whole of the ground-glass stage *G* with a flood of light, reflected from an adjoining incandescent gas lamp or other source of light. As the mirror is carried by the stage and moves with it, the illumination of the field remains unaltered in adjusting the inclination of the pillar to suit the observer. The pillar, stage, and mirror move with stiff friction round the centre *A*, and can be clamped in any position by turning a milled-head screw. In order to prevent any shifting of the observer's head and to avoid fatigue, a hinged and padded head-rest *H* is fixed in such a position that the forehead rests comfortably upon it. The head-rest is also made to rise or fall, and can be clamped at any elevation to suit the observer.

But the use of the Entoptiscope is not confined to the detection and delineation of opacities in the eyeball. The circular image the observer notices on the stage is not an enlarged view of the orifice through which he is gazing, but, as already mentioned, it is a magnified image of his own pupil. The iris limits the divergent cone of rays entering the eye, and its shadow is sharply depicted on the retina. Hence, any irregularities in the iris are at once detected, and can be readily traced by the patient, and the exact size of the pupil accurately determined. *Pupillometry*, as it is called, is one branch of ophthalmology; and the ease, expedition, and accuracy with which it can be accomplished by means of the entoptiscope, will, I hope, render this instrument, in the hands of oculists, a useful means of supplementing, if not of replacing, the usual external methods of observation of the pupil. Even among the limited number who have so far used the entoptiscope, I have been struck with the deformation of the pupil occurring and drawn in one or two cases, though the defect was not conspicuous externally to any casual observer.

I have already mentioned at the outset that it was the discovery of a small permanent obscurity in both of my eyes, before any opacity was detected by the ophthalmoscope, that led me to devise the entoptiscope; and I will now give a recent tracing I have made of these obscurities. [A reproduction of the shadows seen by means of

the entoptiscope, respectively, in my right and my left eye was shown on the screen.] That these obscurities are due to cataract—that is to say, to a thickening of the tissue of the crystalline lens—there is no doubt, as will be evident from the careful drawing of the appearance presented in the ophthalmoscope, which was kindly made for me by the eminent oculist Dr. Charles Fitzgerald. As already explained, entoptic observation inverts both laterally and vertically the shadows on the retina, whereas the opacity is seen without inversion in the ophthalmoscope. A comparison of the two sets of drawings must, therefore, be made after the complete inversion of one or the other. Dr. Ettles was good enough to spend some time in making as minute a drawing as was possible of the cataract in my right eye; and a copy of his drawing was shown on the screen.* The much greater detail shown by entoptic observation is obvious; but the rough entoptic drawing in fig. 1 gives no idea of the wonderful structural detail which the observer sees. The minutest change in opacity is clearly visible; and, as I have said, the effect of any therapeutic treatment of cataract, if such should ever be found worthy of trial, could be rigorously tested. It is, of course, desirable that the entoptiscope should be kept in the hands of the profession; otherwise nervous people would be apt to alarm themselves needlessly by its use. In my own case, if a personal reference will be pardoned, I have found the periodic examination of my own eyes a matter of considerable, if not very exhilarating, interest; inasmuch as the slow progress of the cataract, and the curious way in which it spreads, can be watched up to the inevitable end. The only inconvenience so far felt has been due to the increasing astigmatism which, in my case, began and appears to increase with the development of cataract.

Another, and by no means unimportant, use of the entoptiscope by the oculist, will be found in the certainty with which it will enable him to dispel alarm in very

* I met Dr. Ettles at the Royal Society Conversazione last year, where he was exhibiting that fine instrument the Ettles-Curties Ophthalmometer, and was struck with his scientific enthusiasm and all too rare knowledge of entoptic phenomena.

many cases where a patient, from perceiving some obscurities such as the *muscæ volitantes* in his field of view, imagines he is about to lose his sight. Looking through the entoptiscope, if any muscæ are present, they will be conspicuously seen slowly moving over the illuminated stage; not only will their characteristic appearance and mobility enable the oculist to make an immediate diagnosis, but their actual size and position in the eyeball can be accurately determined. Fifty years ago, Sir David Brewster pointed out the value of entoptic observation in such cases, he remarks:—"Few symptoms appear so alarming to nervous persons as the *muscæ volitantes;* and that these fears can be dispelled by the application of a recondite property of divergent light which has only been developed in our own day is one of the numerous proofs which the progress of knowledge is daily accumulating, that the most abstract and apparently transcendental truths in physical science will, sooner or later, add their tribute to supply human wants and alleviate human sufferings."*

As the *muscæ* occur nearer the retina than the lens, their shadows can be seen, as is a matter of common experience, without the use of a homocentric pencil of rays. Hence a very simple method suggests itself for ascertaining approximately the relative position of any obscurities within the eyeball. This consists in gradually increasing the size of the aperture in the eye-piece of the entoptiscope. Accordingly, a graduated series or orifices is made in the revolving diaphragm of the eye-piece. Upon rotating the diaphragm so that these orifices, from the minutest to the largest, successively come into view, the observer will note a corresponding disappearance of obscurities in the order of their distance from the retina. Finally, with the largest aperture, whilst the definite shadow of even advanced cataract is lost, the far smaller shadows of muscæ, or any obscurity very near the retina, still remain. The fears of a patient, if groundless, can be thus set at rest at once. Retinal defects, if existing, are, of course, still seen, but are easily distinguished from obscurities in the eyeball, and can be examined by retinoscopy.

In order to find the *exact* position within the eyeball

* 'North British Review,' November, 1856.

of any obscurity, the best method is that of employing two minute and closely-adjacent apertures in the diaphragm; two overlapping images of the pupil are thus produced, together with duplicated shadows of the obscurities. As already explained, when the path of the rays within the eye is parallel, the distance apart S of the duplicated shadow is in the same ratio to the distance R of the entoptic object from the retina, as the distance apart C of the centres of the two overlapping pupillary discs is to the distance P of the pupil from the retina, or

$$R = \frac{S}{C} P.$$

The distance P in a normal eye is 19 mm. As C corresponds to the portion of the circles which do *not* over-

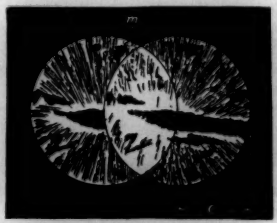


FIG. 11.

lap, a comparison of that distance with S can at once be made. If the entoptic object be near, or on the cornea, its duplicate shadow SS is seen further apart than C; if it be on the anterior face of the crystalline $SS = C$; if on the posterior face SS is rather smaller than C; if near the retina SS is seen well within the overlapping part of the circles, and much smaller than C, in this case the duplicated shadows SS are, in fact, quite close together.

Fig. 11 is a careful tracing I have made of the obscurities seen in my left eye with a double aperture; the distance C corresponds to the distance apart of the centres of the two circles. If the reader will take the trouble to

measure the distance asunder of any of the prominent duplicate obscurities he will find that distance exactly equal to *C*, hence these opacities lie on the anterior face of the lens in the pupillary plane. A small *musca* near the retina is shown, with its double shadow close together at *m*. Owing to the superposition of the two discs of light the overlapping part will be noticed to be brighter than the other portions of the duplicated pupillary disc.

The observer will probably notice that on first looking through the entoptiscope the two circles of light barely overlap, but as the pupil dilates in the subdued light the overlapping rapidly increases, soon reaches a maximum and becomes steady, if the illumination remains unchanged. This is due to the fact that as the pupil dilates its projected image enlarges and the double images or discs of light, therefore, encroach on each other. Hence, before making any measurements a moment or two should elapse to allow for the dilatation of the pupil. The duplicating of the image of the pupil is a very delicate method of measuring changes in its magnitude.

As the Entoptiscope is designed chiefly for the use of the ophthalmic-surgeon, it would be the presumption of ignorance on my part if I entered into its clinical use. But there are several interesting phenomena connected with vision which, I venture to think, will render the instrument of value in physiological and psycho-physical demonstration and research. To some of these I will now briefly allude.

The observer, having adjusted the inclination of the instrument to suit his convenience, moves the mirror until a brilliant and uniform light covers the ground-glass stage. A shaded lamp, or incandescent gas-mantle, or a couple of incandescent electric lights, or the bright light of the sky, can be used as the source of light. It is, however, preferable to employ a constant source of light —such as the incandescent mantle placed at a given distance from the mirror. Both eyes should be kept open, and buried up to their sockets in the eye-cups, the head-rest being moved until the forehead is comfortably supported.

(1) When the observer's eyes are adjusted in the eye-cups, and the brightly lighted stage seen through the smallest apertures in each eye-piece, the graduated sec-

tor, as already stated, at once indicates the exact distance between the pupils, if this is required.

(2) One of the revolving diaphragms is then turned to *O*, to occlude whichever eye is not under observation. Even without a transparent scale in the eye-piece or on the stage, the observer can readily note the well-known fact that, light falling on one eye causes *both* pupils to contract simultaneously. This 'consensual reaction,' as it is termed, and its rate, can be accurately studied with the Entoptiscope. It is only necessary to open suddenly the largest aperture in front of one eye, keeping the smallest aperture before the other, when the contraction of both pupils will be seen. Still better, if the stage be dimmed by altering the mirror, an electric lamp brought close under one eye with the largest aperture open, though not seen by the other eye, nevertheless causes a vigorous contraction of its pupil. The scale, or the ruled squares on the tracing-paper, enables the contraction to be measured, whilst the periodic time of the consensual reaction can be determined by opening and closing the large aperture in front of the electric lamp.

(3) The change of the pupil, during accommodation, has already been mentioned. This 'accommodative contraction,' as well as the direct light reaction in the pupil, is strikingly seen in the Entoptiscope; turn both diaphragms to *O*; then, after a few moments in darkness, open an aperture in one eye-piece. The contraction of the pupil from light-reaction is first seen. If a pencil-point be now brought in the field of view on the stage and looked at, accommodation takes place, and the pupillary disc will be seen to contract; the pencil may be suddenly raised near the eye-piece, when a further contraction occurs owing to the convergence of the eyes that now takes place.

Other causes which give rise to pupillary contraction, such as an emotional disturbance, fright, pain, and the action of certain drugs, etc., can be studied more readily, and their effect may be, I believe, more accurately measured by the entoptiscope than in any other way.

A magnified view can also be obtained of a slight rhythmic contraction of the pupil (said to be seen by some, though I have not noticed it), which appears to be connected with respiration and the systole of the heart.

(4) One of the most striking phenomena observed with

the Entoptiscope is the extraordinary displacement of the projected image of the pupil which takes place when it is seen first by *direct* and then by *oblique* vision. This change in the position of the image, due to a change in the point of view, is not the ordinary parallactic displacement, but is an *ocular parallax* due to the structure of the eye, the centre of notation of the eye not corresponding with the optical centre, or "centre of regard." It will be sufficient here to describe a few experiments on this parallax, which can be made with the Entoptiscope.

(*a*) Look through one eye-piece; bring a pencil-point as at *b*, fig. 12, to the very edge of the projected pupillary disc, looking directly at the pencil. Without moving the pencil or the head, turn the eye to the opposite edge *a* of the pupillary disc, the pupil will appear to have suddenly expanded to *b'*, as shown by the dotted lines, and the pencil will, therefore, be now seen well within the disc; but, when again the eye turns to *b*, the disc resumes its first position on that side, and the side *a* correspondingly expands.

(*b*) Now shift the pencil outside the disc, between *b* and *b'*, as shown by the dot, so that it cannot be seen if directly searched for; when, however, the eye is turned away from it to *a*, instantly it reappears; this occurs equally well at either side of the disc, with either eye. This paradoxical effect of *seeing an object when you look away from it, and not seeing it when you look directly towards it*, is not due to the image falling on the blind spot, for it occurs, as I have said, at either side of the disc, or at any portion of the retina oblique to the axis of vision.*

One plan of avoiding this troublesome parallax in the Entoptiscope is to use a very small artificial pupil in

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* A paper by Sir David Brewster in the Trans. Roy. Soc., Edinb., for 1843, vol. xv., p. 349, contains the first reference I can find to ocular parallax, which Brewster appears to have observed in cases of oblique incidence in ordinary vision. Subsequently Listing investigated the matter, *Beitrag zur Physiologischen Optik*, 1845, p. 14 *et seq.*, and gave the true explanation already mentioned.

the eye-piece, close to the eye; or, still better, a small diaphragm below the eye-piece.

(5) Upon winking the eyelids, transverse striæ will be perceived on looking through the Entoptiscope, due probably to minute wrinkles in the epithelial layer of the cornea. Rubbing the cornea causes the roughening of the epithelial layer, which gives rise to a peculiar ribbed or mottled appearance in the field of view of the Entoptiscope, but this soon disappears. Dr. Thomas Young was, I find, the first to notice and depict this, so long ago as 1801.

(6) But more important is the study of the structure of the crystalline lens as seen in the Entoptiscope. The observer will probably notice a star-like figure radiating from the centre of the field of view. This is due to slight differences in refractive power of the fibrous tissue separating the sectors into which the crystalline is known

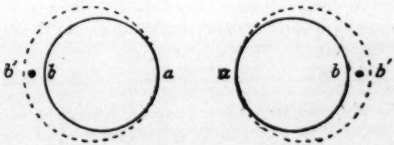

FIG. 12.

to be divided. In his wonderful paper on the "Mechanism of the Eye," published in the Philosophical Transactions upwards of a hundred years ago, Dr. Thomas Young gave a drawing of the stellate structure of the human crystalline. Bright spots, due to higher refrangibility, are also frequently seen on the crystalline, these are termed "pearl-spots."

Listing, and subsequently Donders, classified the various entoptic appearances seen in the crystalline and the anterior part of the eye. Excellent drawings of these are given by Donders and by Helmholtz. But, as already remarked, it is surprising that none of these eminent men noticed the great value of entoptic observation in the case of cataract. I cannot lay claim to the first discovery of this, for though it was independently discovered by myself, Dr. Darier, of Paris, appears to have

been the first to draw attention to this matter some ten years ago.

(7) Reference has already been made to the ease with which the Entoptiscope enables the *muscæ volitantes* to be seen and their position in the eyeball found. In this case a large pin-hole aperture should be used, in order to give a large retinal illumination, and the pillar of the Entoptiscope should be fixed vertically, so that the eye looks directly downwards. The muscæ will now be seen nearly stationary, for being rather lighter than the vitreous humour they will slowly ascend, and thus their motion being in the line of sight their shadows will not be displaced, but become clearer as they approach the retina. Dr. Jago, in his "Entoptics," gives some careful drawings he made long ago of different types of muscæ. Helmholtz, in his "Physiological Optics," also gives some excellent drawings of the muscæ, and divides them into four or five groups according to their appearance.

(8) The Entoptiscope also enables the "light-sense" of an observer to be tested, and the threshold of visibility of each eye determined, *i.e.*, the degree of illumination which forms the lowest limit of visibility—the *lux-liminal* point it might be termed. For this purpose the smallest aperture may be used in the case of a normal eye, and a small steady source of light (such as a night-light) is shifted to different distances from the mirror of the instrument in a well darkened room. The limiting distance is then measured, and, as the illumination of the ground glass stage follows the law of inverse squares, the light threshold is at once found, relatively to some standard.

(9) As already explained, a minute object can be clearly seen when held close to the eye and viewed through a stenopaic screen. Under such circumstances a highly magnified image of the object is perceived from the large visual angle subtended by the object, whilst the blurring of the image on the retina is prevented owing to the circles of diffusion being reduced to mere points. The Entoptiscope not only affords a most convenient way of exhibiting this magnification of a microscopic object without the aid of a microscope, but also enables the observer to make an accurate tracing of the magnified image of the object on the ground glass stage. A bril-

liant source of light, the smallest aperture in the diaphragm, and the shortest eye cups should be used.

(10) As the refractive index of the cornea is nearly the same, and that of the aqueous humour the same, as water, by immersing the eyes in water, refraction by the crystalline alone becomes effective. This method was employed by Dr. Thomas Young to demonstrate that accommodation was effected by a change in curvature of the crystalline.* The experiment can easily be made with the Entoptiscope. The pillar of the instrument is clamped vertically, and an eye cup, fitted with a glass diaphragm below, is placed in position. The cup is nearly filled with (not quite cold) water, and the eye immersed, so that the cornea is in contact with the water. Open the largest aperture (say 2 or 3 mm. diameter); note: (*a*) the focal length of the convex glass lens requires to be held below the aperture in order to restore clear vision; the refraction by the cornea is thus seen to be far greater than that by the crystalline (as 5 to 2); this is owing to the latter being immersed in media only slightly differing from itself in refractive power; (*b*) keeping the selected convex lens before the aperture, note that accommodation still takes place; (*c*) changing the aperture to the smallest size, note that clear vision is now restored without the use of any external glass lens.†

Immersion of the cornea in water also enables the observer, if astigmatic, to note whether any residual astigmatism is due to the crystalline lens, or whether any power of astigmatic accommodation by the lens can take place, as some think does occur.

Doubtless other applications of the Entoptiscope will occur to the ophthalmologist. One of the simplest is its use in testing astigmatism by the appearance presented

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* *On the Mechanism of the Eye.* Phil. Trans. for 1801, p. 23.

† For my own part I cannot understand why evolutionary processes did not lead to increased sensitiveness of the retina and a pin-hole eye like that of the nautilus only with a protecting cornea. Such an eye would have many advantages over our own; no accommodation would be required; no refraction errors would occur; no spherical or chromatic aberration would exist, and no cataract would occur.

by the luminous point, when the eye of the observer is gradually removed from, say, 3 to 30 inches. The observer will notice in this experiment how the slightest pressure on the cornea completely alters the appearance of the elliptical diffusion-spot, or other image, which the luminous point assumes in astigmatic eyes. Tscherning goes so far as to say there is no optic defect which is not shown by means of the figures presented by a luminous point.

Discussion on Professor Barrett's Paper.

The Chairman said the wrapt attention with which they had listened to Professor Barrett proved how well-appreciated the lecture had been. There was only one drawback, which was that the Professor had given them so much to think over and digest in connection with this almost unknown subject of entoptic vision that the time for discussion was somewhat shortened. However, as they had with them one or two gentlemen who had studied this subject he would not detain them with any remarks of his own just then.

Dr. Ettles rose to open the discussion as follows:—

The able and most interesting address to which we have had the pleasure of listening to-night offers so few opportunities for criticism, that I feel unable to venture on any, nor, at this late hour, would it be advisable to follow up the side issues, such as the mental projection of retinal impressions, for by doing so we should lose sight of the valuable material brought together by the lecturer. There are two apparently uncharitable thoughts which are uppermost in my mind just now. One is, that, hearing it seriously suggested that I should give this address on Professor Barrett's behalf, I so obstinately refused that he had to make this long journey to do it himself, and although I must apologise for causing him this inconvenience, I am sure you will all be grateful to me for having got him over. The other is thankfulness for his cataract.

Had it not been his accidental seeing of the striae under suitable conditions, this work on his part would probably never have been done. It is but one of many instances where a skilful observer's infirmity has been turned to most excellent account. But though we are not sorry in this sense that his lens has opacities, we

sincerely hope they will remain stationary. To utilise the greeting of his talented countrymen, we would say, "may his bodily shadow never grow less and his entoptical shadows never grow greater."

In his concluding remarks, Professor Barrett pointed out one of the resources of his beautifully designed apparatus, which had not occurred to me, although I have used it for some time. I refer to its power of rapidly measuring the light sense. In that sense we recognise two divisions: the light minimum, or "threshold," and the sense of light difference.

Now, it has been found that this "threshold" possesses great clinical value. In some of the more obscure diseases of the optic nerve behind the ocular bulb, where there is no objective evidence of disease, it has been found that a displacement of the point of liminal intensity is almost completely diagnostic. Hitherto the estimation of that point has been a very complicated matter, so much so that the busy practitioner often lets it go unsolved. Here, however, is a method by means of which it can be done in a few minutes, and that may mean a great deal in the proper treatment of disease.

One of the applications of this instrument for which I confidently predict a great future, is as follows: You all know that up to the present there is but one way of combating cataract, by "cold steel." It may well be that the time will come when some drug or agency may be discovered which will have the power of causing the absorption of these striae. In the search for that agency, and in the recording of its results, we have in the entoptiscope a perfectly ideal resource.

So far as the localisation of opacities is concerned I would like to give you a very simple working rule. Take a card, cut it round to fit into the trial frame, and puncture its centre with a pin. The hole you want is only 1/10th of a millimeter, so just the point must pass through. An ordinary pin is about half a millimeter thick, and feels about ten millimeters if you happen to sit on it. Next, take a candle and hold it near the hole placed at the anterior focal plane and move it up and down. Those spots moving with the flame are seated in front of the pupil, while those moving against the flame are situated behind it. I think the next best thing to making a discovery, is to discover the real value of an

unappreciated fact found out by someone else, and by expanding it and convincing the world of its usefulness to bring about the general recognition of its worth. That is what Professor Barrett has done.

I do not think he lays claim to the discovery of all these points, but we are profoundly grateful to him for having properly appreciated them, for having hunted through the entire literature connected with the obscure problem in physiological optics, and presenting to us so valuable a bibliography, and, lastly, for having laid before us so many valuable suggestions which are sure to bear fruit in the future transactions of the Society.

Mr. J. W. Gordon said he was very much obliged to the Society for the opportunity afforded him of being present, and he would like to associate himself with the Chairman and Dr. Ettles in their appreciatory remarks on the extraordinarily interesting lecture which Professor Barrett had delivered. He was already indebted to the Professor for the opportunity of reading his papers and making himself more or less acquainted with the subject. What struck him was not so much the great interest attaching to this subject as the amount of original work done upon what was intrinsically obscure although it lay, in a sense, on the very surface. It had remained for Professor Barrett to work out what he might call the scientific aspect of the matter.

They were indebted to him for a knowledge of a great deal of fragmentary work done in previous years. In spite of what had been done by such men as Dr. Young the subject had been allowed to rest until taken up within the last few months by Professor Barrett, and carried through in what struck him (Mr. Gordon) as being an extraordinary instance of successful original work. He would like to return his thanks to the Professor for the pleasure obtained in listening to the lecture which had so splendidly illustrated a subject of very great importance. (Applause.)

Mr. Aitchison said, as a London Optician and an official of the Optical Society, he should certainly like to associate himself with the vote of thanks to Professor Barrett for his kindness in coming from Dublin and giving them such a very interesting paper. They all knew the immense value of pins in our civilisation, but it had now been made clear to them of how much greater

importance was the hole that the pin could make. In regard to Dr. Ettles' remarks that pathological conditions did not come within the sphere of Opticians, in a certain sense he was right, but in another sense wrong, because investigations such as the one described by Professor Barrett, which enabled them to detect morbid conditions, were of very great value to the Optician and to the surgeon, because it enabled the Optician to avoid the pitfalls which had been the cause of so much friction between the Optician and the ophthalmic surgeon. The older order of the ophthalmic surgeon had an idea that the whole aim and object of the Optician was to encroach upon the surgeon's profession. On the contrary, the aim of the Optician was to avoid trespassing upon the rights and privileges of the ophthalmic surgeon, and the closer intercourse between the two sections would lead to more satisfactory results in every way.

Personally, he believed Opticians had done, in a way, nearly as much as ophthalmic surgeons, towards showing the necessity of wearing spectacles in correcting defects of vision, and that a closer intercourse would remove many of those causes of trouble which existed at the present day. He thought that such an instrument as Professor Barrett had introduced to their notice, and the information given them, would all tend towards that end. He hoped that they would soon have the pleasure of hearing Professor Barrett again. (Applause.)

Mr. Edser said he was delighted to have the opportunity of expressing his admiration of the work which Professor Barrett had done. There was one experimental result which he should like further to recommend to Professor Barrett's notice, as if it was really a true one it appeared to him to be very important in connection with the theory of vision. It was well known, of course, that if you put a pin-hole in front of your eye and looked at a white globe while moving the pin-hole about you saw the shadow of the retinal blood-vessels.

Koenig had given a short account of a further development of this experiment. He used monochromatic light and two pin-holes, and, of course, observed two shadows of the retinal blood-vessels, when the pin-holes were kept in motion. By observing the distance between these two images, the distance from the retinal blood-vessels to the sensitive layer of the retina could be de-

termined in practically the way that Professor Barrett described. But, acording to Koenig the distance between the sensitive layers for the blue and the red is much greater than the thickness of the layer of rods and cones.

From this Koenig concluded that the sensitive layer of the retina for red light is not in the layer of rods and cones at all, but in the pigment layer of the choroid. If the results that Koenig had obtained could be relied upon it appeared to him (Mr. Edser) to be a point that had been neglected in recent years. A great deal of confusion had been created, the study had been left to psychologists, and physicists had, to a great extent, lost sight of the matter. He had made a few experiments himself, but they are not very conclusive.

The chief difficulty appeared to be that the eye is very much affected by chromatic and spherical aberration, and these defects might produce the effects observed by Koenig. What would be most satisfactory would be to have an instrument to measure the focal lengths of the living eye for different colours. It, of course, could be done in the dead eye, but it had not been conclusively proved that the two were the same. There were numerous other points he would like to have touched upon, but he would content himself with these few remarks, and once more express his gratitude to Professor Barrett for making this interesting subject so clear. (Applause.)

Mr. S. D. Chalmers said he would like to join the other members in expressing thanks to Professor Barrett for his lecture. He would like to call attention to one or two small matters in connection with the diagrams they had seen which would enable them to realise exactly how some of the observations were made. While the slides were being thrown on the screen he had observed a beam of light between the lantern and the screen, and various objects were interposed. Once the pointer came across this beam of light, and they had the shadow thrown upon the screen. Then, again, a member of the audience was smoking right in the beam of light, and there could be seen on the screen the shadow of the smoke. It was an interesting illustration of what Professor Barrett was describing.

He (Mr. Chalmers) had not had any actual experience of this method in connection with visual work, but he

was often engaged in testing lenses, and had used a method where one projected on a screen images of diagrams placed in contact with the lens. It might be possible for investigations of the various aberrations of the eye to be carried out by means of a comparatively small modification of Professor Barrett's method. It might be possible by using a different distance between the pin-hole and the eye to draw out the shape of the projection of the pupil of the eye in various positions. It might be necessary to use a second diaphragm just in front in addition to the small pin-hole necessary for the actual entoptic phenomena to be seen.

Professor Barrett said he wished to thank the gentlemen who had spoken for their very kind remarks. He would like to call attention to the fact that Mr. Gordon was the discoverer of a very valuable method for use in higher-power microscopy. He had succeeded in removing the defects produced by specks of dust in the lens. The point of light was allowed to fall upon a small piece of ground-glass, and by a small motor to be set in motion, and all the trouble produced from entoptic phenomena in higher-power microscopes was thus eliminated. (Applause.)